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## THE TRIAL OF GALILEO.[1]

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MANY points have been left obscure in the history of the double trial of Galileo, the details of which till lately were but imperfectly known. The important work published by Domenico Berti[2] fills up some of these gaps, by placing before our eyes a collection of authentic documents taken from the secret archives of the Vatican. We have here no work of partisanship, undertaken in the interest of religious controversy, but an historical work executed with all the scrupulous care that is nowadays exacted in all historical researches. In France the question had already been handled by Libri, Biot, Joseph Bertrand, Trouessart, and Th. Henri Martin, the first two approaching it with preconceived opinions that aided but little in the discovery of the truth, while the others brought to the discussion a remarkable spirit of impartiality. But all of these writers lacked the indispensable elements of information which now, thanks to the labors of Domenico Berti, are at the disposal of the future biographers of Galileo. If we have suffered ourselves to be anticipated by an Italian in the publication of the documents relating to this famous case, we must attribute the fact either to the negligence or to the prudence of the French Government, for we were in possession, for nearly half a century, of the valuable manuscript the full text of which is now published by Berti. Having been taken out of the Vatican archives during the first empire and carried to Paris, this original collection was there seen by the historian Denina, but he thought it to be of no importance. Nevertheless Napoleon I. ordered it to be published, with a translation facing the text; but the publication, though begun,

[1] Translated from the *Revue des Deux Mondes* by J. Fitzgerald, A. M.

[2] "Il Processo Originale di Galileo Galilei, pubblicato per la prima volta," Roma, 1876.

was not continued: only the beginning of the work was then known, and of this Delambre, the astronomer, gave an account to the Italian Venturi.

In December, 1814, according to the Duke de Blacas, the private library of Louis XVIII. received the entire MS. in the same condition in which it had been found by the imperial government in Italy, and thence carried to France. During the early years of the Restoration, active negotiations were carried on by the court of Rome with the French Government, to secure the return of these important documents. The Government, though it did not positively refuse to comply, nevertheless delayed and procrastinated. It was not until 1846, after thirty-two years of negotiation, that the MS. was sent back to Rome, at the instance, no doubt, of Rossi, who himself presented it to Pius IX., in behalf of Louis Philippe. By the pope it was restored in December, 1848, to the secret archives of the Vatican, and there it still remains.

All that was known of this MS. before the publication of Berti's work rested upon a selection of documents published at Rome in 1850, with many precautions, by Monsignore Marino Marini, sometime Prefect of the Secret Archives of the Holy See, and upon a larger work, in some respects inexact, and in others imperfect, published in Paris in 1867, by Henri de l'Épinois. Both of these writers take special points of view: they appear to be more intent upon justifying the judges that condemned Galileo than upon laying bare the whole truth with the boldness and freedom of an historian. Hence we can appreciate the motives which led them to publish only a portion of the MS. though the whole of it was in their hands. Did the court of Rome really suppose that these two publications contained all the documents pertaining to the double trial of Galileo, or did it think that the time had come for no longer hiding anything from the public? However that may be, at all events Domenico Berti, in February, 1870, was permitted to examine the MS., and even to copy it at his leisure in the room of Father Theiner, who had been officially authorized to intrust it to him. The present publication, therefore, was not procured by fraud, and, if the Holy See should have any occasion to regret it, at least it could neither dispute its authenticity, nor complain that the work was done without its consent.

I.

The interesting history of the travels and of the final destiny of the Vatican MS. is merely the preface of a far more important history, whose events we will endeavor to record impartially, with the sole purpose of unveiling and bringing to light the truth. Galileo, celebrated from his early years for the value of his discoveries and the brilliancy of his lectures at the University of Padua, loaded with honors at Venice and at Florence, and admired throughout all Italy,

was pursuing the course of his great researches with the boldness of a man confident of his strength and of his fame, when certain slight indications no doubt warned him that it would not be disadvantageous, if he would carry on his researches in safety, to win the favor of the Sacred College. Accordingly he set out in 1611 for the Eternal City, without confessed misgiving, but with the ambition and expectation of interesting the most influential personages of the Roman court in his discoveries. He was nearing the decisive moment of his career. He had not as yet been disquieted by the objections of the theologians, though in prosecuting his studies of the constitution of the universe he was touching upon delicate questions which he could not expect to be permitted to discuss freely, without having first gained the sympathy, or at least the neutrality, of the Church. The court of Rome at that time exercised such moral authority in Italy, and especially at Florence, where Galileo resided, that people in some sense waited for her decision before they would accept the best-established conclusions in astronomy. The Grand-duke of Tuscany could not but be pleased at the discovery of Jupiter's satellites, announced in the "Sidereus Nuncius;" and he was all the more ready to believe, because these new heavenly bodies had received his family name: yet his own secretary had to admit that the discovery would never receive the unanimous assent of the learned world until it was approved and verified at Rome. There sat the Roman College, a regular tribunal, scientific as well as theological, whose decrees were law in Catholic countries.

Galileo, who was a man of rare good sense, and perfectly conversant with the ways of the world, had in advance formed at Rome the best and the most powerful of relations. Besides, he came there in a sort of official capacity, at the grand-duke's charges, and he was entertained there by the Tuscan embassador. Prelates, cardinals, princes, vied with one another for the honor of offering *fêtes* and banquets to the most illustrious representative of Italian science. At the palace of Cardinal Bandini, in the beautiful gardens of the Quirinal, in the villa of the Marquis Cesi on the summit of the Janiculan, Galileo delighted a society of *élite* by having them contemplate, during the serene nights of April, the vault of heaven through the telescope which he had recently invented, and which bears his name. He awakened a genuine enthusiasm one day when, after dinner, he pointed his telescope toward St. John of Lateran, three miles distant, and enabled the guests to read the inscription upon the façade of that basilica.

His arguments did not equally convince all of those who were present at his astronomical observations, and who listened to the explanation he gave of the movement of Jupiter's four satellites, the inequalities of the moon's surface, and the phases of Venus and Saturn, and to the discussions he carried on with those who opposed his views. His

doctrine implied the confirmation of the system of Copernicus and the demonstration of the earth's motion, which were no longer reserved for mathematicians only, but made intelligible to all by a series of experiments. Here was an innovation calculated to alarm the theologians. A system that might be regarded as inoffensive so long as it was only a mathematical hypothesis, useful to men of science in their researches, became a very different thing on being transformed into a physical truth accessible to the senses and pregnant with consequences touching the plurality of worlds and the aim of creation. Hence the apparent triumph of Galileo hid from view perils the magnitude of which at first eluded his penetrating mind. While he was giving himself up, with perhaps over-much confidence, to the pleasure of success, and was yielding too easily to his habitual temptation to answer objections with sarcasm, the ecclesiastical authority quietly set on foot an inquiry into the orthodoxy of his opinions. Cardinal Bellarmin, probably in the name of his colleagues of the Inquisition, asked of the members of the Roman College (without mentioning Galileo's name) what was to be thought of the astronomical observations that had recently been promulgated by a distinguished mathematician.

This is the first symptom that we have been able to discover of the intervention of theology in the examination of Galileo's scientific opinions. The response of the Roman College was favorable to him; but, from that moment forward, the alarm was sounded, and the Inquisition never lost sight of him. Though the sovereign pontiff, to whom he was presented by the Tuscan embassador, received him with great courtesy, not allowing him to utter even a word on bended knees, yet the Holy Office, even before he had quitted Rome, inquired of the tribunal at Padua whether, in the action brought against Cesare Cremonini for certain philosophical indiscretions, there might not be something to compromise Galileo. A direct personal attack, inspired by an overweening zeal, quickly followed these early suspicions. On his return to Florence, Galileo took up his labors afresh in the pleasant solitudes of the Belvedere, placed at his service by the kind hospitality of the grand-duke; there he received his friends and pupils, who, on departing from these *conversaziones*, propagated his doctrines. At this a Dominican friar, Thomas Caccini, took umbrage, and, in a sermon delivered at Santa Maria Nuova on the miracle of Joshua, he suddenly exclaimed, "*Viri Galilæi, quid statis aspicientes in cœlum?*" The friar doubtless had heard of a conversation held at the court in presence of the grand-duchess dowager Christine of Lorraine, and the Archduchess Madeleine of Austria, in the course of which Father Castelli, a pupil of Galileo, had endeavored to prove, to the great satisfaction of his hearers, that one might believe in the earth's motion without questioning the authenticity of Joshua's miracle. Upon this subject Galileo addressed to his pupil a famous letter, in which he

precisely set forth the rights of science, at the same time asserting for religion its own. Here, according to him, are two separate domains, which are not rashly to be confounded.

"The Holy Scripture," said he, "can neither lie nor err, but it needs to be interpreted; for, were we to insist upon the literal sense of the words, we should find not only contradictions, but heresies and blasphemies; we should have to give to God hands, feet, ears, to suppose him subject to like passions with men—to anger, remorse, hatred; and, again, to hold that he forgets the past and is ignorant of the future. . . . Inasmuch as the Bible constantly requires interpretation to explain how very different the true sense of the words is from their apparent signification, it appears to me that it should be quoted in scientific discussions only as the last resort. In truth, Holy Scripture and Nature both come from the Divine Word, the one being the dictation of the Holy Ghost, while the other is the executor of God's decrees; but it was fitting that, in the Scriptures, the language should be adapted to the people's understanding in many things where the appearance differs widely from the reality. Nature, on the other hand, is inexorable and immutable; she is not at all concerned whether the hidden reasons and means through which she works are or are not intelligible to man, because she never oversteps the limit of the laws imposed upon her. Hence it appears that when we have to do with natural effects brought under our eyes by the experience of our senses, or deduced from absolute demonstrations, these can in no wise be called in question on the strength of Scripture texts that are susceptible of a thousand different interpretations, for the words of Scripture are not so strictly limited in their significance as the phenomena of Nature. . . . I therefore think it would be wise to forbid persons from using texts of Holy Scripture, and from forcing them, as it were, to support as true certain propositions in natural science, whereof the contrary may to-morrow be demonstrated by the senses or by mathematical reasoning."

This noble letter, the moderation of which would nowadays be admitted by every theologian, but which then gave out a dangerous odor of novelty, no doubt passed from hand to hand, was read by ill-disposed persons, perhaps fomented the agitation produced by Caccini's vehement assault, and furnished to another Dominican, Niccola Lorini, an opportunity of denouncing Galileo to the Congregation of the Holy Office. "Here," said the informer, "are propositions that seem to be suspect and rash, opinions that contradict the text of the Holy Scripture. Besides," he added, "Galileo and his disciples speak with little respect of the fathers of the Church, of St. Thomas of Aquino, and of Aristotle, whose philosophy has rendered so much service to the scholastic theology." The Inquisition, though search was made, was unable to procure the original of the letter, for Castelli had given it back to his master, and he prudently refused to part with it. The

inquisitors contented themselves with an examination of the copy sent by Lorini, in which they discovered a few ill-sounding phrases, but, on the whole, nothing clearly contradictory of the language of Scripture. Still they continued to note the words of Galileo; they questioned two Tuscan ecclesiastics about the speeches that he might have uttered in their hearing; they scrutinized the letters he had published on the subject of observing sun-spots.

Galileo, though quite ignorant of the strict watch kept on him by the Inquisition, had a vague apprehension of imminent danger. To ward it off, he adopted the expedient of going again to Rome in 1615, and of pleading his cause in person in the quarter where a successful defense was most to be desired. It has been asserted that Galileo was summoned before the bar of the Holy Office, but they who so assert are in error as to the date; it was not till a much later period, viz., the beginning of his second trial, that he was ordered to appear in Rome. On the present occasion he went of his own accord, no longer possessed of the fearless assurance with which he made his first journey, yet confidently hoping that he would disarm his opponents by the clearness of his explanations. Perchance he rested his expectation of convincing them as much upon the graces and charms of his wit, and the personal attractiveness which won for him all hearts, as upon the strength of his arguments.

Besides, he had taken more pains than even he did in 1611 to prepare the ground: he had, in urgent letters, rekindled the zeal of his friends, and had again obtained for himself all the external tokens of the official protection of the grand-duke. As before, he went down to the embassador's palace, the villa of the Trinità de' Monti, where now the Academy of France has its seat, and, the day after his arrival, went into the country. What with detailed explanations made in the presence of numerous auditors, keen and lively disputations in which he plainly showed the weakness of his opponents, frequent visits to distinguished personages, brief tractates in which he demonstrated the truth of the Copernican system, he omitted nothing that could influence in his own favor those currents of opinion which judges themselves cannot withstand.

Unfortunately for Galileo, the tribunal of the Inquisition was but little affected by external influences; it imposed laws on opinion, and took no advice from it. The members of the Holy Office, heedless of the steps taken by the illustrious astronomer, and of the ardor with which his ideas were espoused by a portion of Roman society, went on quietly with their work. In examining the letters on the sun-spots, they found therein two propositions worthy of censure. On the 24th of February, 1616, they unanimously pronounced it absurd and heretical to assert that the sun is motionless, and that the earth revolves. The sovereign pontiff immediately ordered Cardinal Bellarmin to summon Galileo, and to have him promise that he would no longer uphold

a proposition condemned by the Church. "If he refuses to obey," said the pontifical letter, "the Father Commissary, in the presence of a notary and witnesses, shall enjoin him absolutely to abstain from teaching that doctrine and that opinion, from upholding it or even speaking of it; in case he does not comply, he shall be cast into jail." Accordingly, on the 26th of February, 1616, Cardinal Bellarmin, in the presence of the commissary-general of the Holy Office and two witnesses, invited Galileo to renounce the two condemned propositions. After Bellarmin, the commissary-general again intimated to him, on behalf of the pope and the entire Congregation of the Holy Office, the formal order no longer to uphold, teach, and defend this opinion, whether by writing, by word of mouth, or in any manner whatsoever; if he failed to comply, he was to be prosecuted by the Holy Office. Galileo promised to obey.[1] On the 5th of March following, the Congregation of the Index condemned the work of Copernicus until it should be corrected.

From these authentic facts it results that a certain number of modern historians are deceived themselves, or would deceive us, when they insinuate that the Holy Office meant to condemn, not the system of Copernicus, but Galileo's theological interpretations of it. There was no question whatever about theological interpretations. In neither Copernicus's book, nor in the letters on the sun-spots, is there a word, a single phrase, in which the Holy Scriptures are interpreted. If here and there in his correspondence Galileo, out of respect to religion, endeavored to reconcile the data of science with the text of the Bible, he never published these explanations. It was not upon these private manuscript documents that he was tried, and the only document that furnished a basis for the charge was a printed work, purely scientific in character, and having nothing whatever to do with theology. By no manner of argumentation can the fact be negatived that a tribunal of theologians constituted itself a judge in a question of science, and decided it as an authority decides. The Holy Office did not forbid receiving and teaching the doctrine of Copernicus, on the ground that it was not yet demonstrated, as some of the apologists of the Holy See would have us believe; they would not permit it to be demonstrated; they pronounced it in advance to be "absurd, heretical, contrary to the text of the Scripture." Such is the whole truth about Galileo's first trial, and Domenico Berti sets it forth with much dialectic vigor.

## II.

Galileo once reduced to silence by the act of submission to which he had subscribed, the object of the Inquisition was attained. No

[1] In a work entitled "Galileo Galilei und die Römische Curie" (Stuttgart, 1876), Herr von Gebler disputes the authenticity of the document which states these facts. Berti makes a victorious reply to him.

useless rigor followed the first procedure. Provided that the culprit spoke no more about the motion of the earth, the court of Rome would like nothing better than to make the most of a great mind that for a moment had gone astray, but whose genius and whose scientific fame were intact. After the trial, Galileo remained three months in Rome, and was kindly received by the sovereign pontiff. In fact, the rumor having spread that he had been punished by the Holy Office and obliged to retract and to do penance, he obtained from Cardinal Bellarmin a certificate to the contrary effect. All that was done, said the cardinal, was to forbid him defending or upholding the system of Copernicus. What advantage could it have been to drag Galileo down from the high position he occupied in the world's opinion? It was enough, for the purposes of his judges, if they could shut his mouth.

In this they supposed they had succeeded, but here they failed to take account of the overmastering impulse to propagate truth, which is the very essence of scientific genius. Galileo could neither erase from his mind a belief that rested on a demonstration, nor refuse to employ it in advancing to fresh discoveries, nor abstain from speaking of it with those who consulted him with regard to their own astronomical labors, or took an interest in his. In his retirement at the Belvedere, where, since his return from Rome, he led a more secluded life than ever, he received, as in former times, numerous visits, nearly all prompted by the love of science. He was still the recognized and admired head of the scientific movement in Italy. Why should he not converse about the cardinal proposition of the earth's motion with the young *savants* who came to ask his advice and to receive his instruction? A distinguished Italian narrates how, having spent a few days with him, after the close of his first trial, he heard from Galileo's mouth the exposition of the Copernican system, was converted to his ideas, and himself then converted Campanella to that doctrine.

Hence the submission of Galileo was only apparent. Later he was justly charged with having broken his promise. Still, he avoided compromising himself publicly, and in his first work, "Il Saggiatore," which is a model of keen, clever irony, he hardly ventured to write anything touching on the system of Copernicus. Presently the election of a new pontiff inspired him with the hope that the court of Rome might relax its rigor. Urban VIII., of the family of Barberini, was a Florentine, a lover of letters, well disposed toward the Academy of the Lincei, and especially friendly to Galileo, to whom he had addressed, while yet a cardinal, some verses conceived in a vein of eulogy. Galileo went to Rome to see him, had six long audiences with him, was presented by him with a picture, medals, *agnus deis*, and a pension for his son, and doubtless talked with him about the great subject which filled his mind. We can only guess at what was

said by the two friends: some authors assert that Urban VIII. then inclined toward the Copernican system; others, on the contrary, say that he demonstrated to Galileo the impossibility of maintaining the theory of the earth's motion. The truth is, that we know nothing about the matter; neither the pope nor the astronomer has given out anything about the nature of their conversations. Perhaps even, as we shall shortly see, they believed that they could agree, while differing from one another widely.

At all events, it seems that, dating from the accession of Urban VIII. to the pontifical throne, Galileo felt more free to touch anew upon the forbidden subject, under a different form. Was this the result of an overweening confidence in the friendship of the sovereign pontiff, of a too favorable interpretation of some friendly speeches, or of the impossibility of being silent while Kepler was speaking boldly outside of Italy, while on Italian soil one was constantly harassed by ignorant opponents, and, though one's hand were full of truths, one durst not open it and rout them. The "Dialogues on the Two Great Systems of the Universe," which were destined to bring Galileo into so much trouble, show that, in writing them, he stood between the conflicting influences of a strong desire to speak and the fear of compromising himself. He rather insinuates his ideas with true Italian finesse than puts them forth boldly. He does not defend the Copernican system, but expounds it. He even takes the precaution of stating, in a preface, the rough draft of which had been sent to him from Rome, that the true aim of his work is to show that in Italy ideas are not condemned unknown, and that nowhere is this delicate matter better understood than in Italy. He carefully avoids drawing conclusions: the personage whom he introduces as the representative of the doctrine of Ptolemy and as the defender of the belief in the earth's immobility, though clad in the strongest dialectical coat of mail, and though driven to his last ditch by the keen raillery and the copious logic of his interlocutors, replies to them unmoved: "Your arguments are the most ingenious that can be conceived, but I consider them to be neither true nor conclusive." Father Riccardi, Master of the Sacred Palace, whose business it was to examine Galileo's manuscript, suffered himself to be half-way won by these exhibitions of innocence, and gave a permit for the work to be printed, though not without resistance. He afterward protested that he had been deceived by the author, and that some of the conditions on which he had granted the *imprimatur* were not fulfilled. At first it was agreed that the "Dialogues" should be printed at Rome; but at the earnest entreaty of Galileo leave was granted to have the work done at Florence, where it would involve less trouble and cost to him, and where, above all, he could more easily evade the surveillance of the Sacred Palace. In this negotiation Galileo displayed a fecundity of resource and a force of will that show how

important he considered the publication of his work to be. The chief fruit of his address was that he escaped a second revision of the text, which would have been made at Rome had the work been printed there. Galileo chose rather to deal with the inquisitor at Florence, to whom Father Riccardi had delegated his powers, but who, doubtless at the solicitation of the grand-duke, exercised these powers with less rigor than would have been used at the Sacred Palace. We can imagine the wrath manifested by the court of Rome; in fact, despite all its finesse, it had been outwitted by an Italian shrewder even than itself, by a fellow-countryman of Macchiavelli.

Would Galileo have been so eager for the publication of his work, if he had foreseen the dangers to which he exposed himself by pub lishing it? The sovereign pontiff, immediately upon receipt of the book, in the beginning of August, 1632, was highly incensed, charged Galileo with having made an unhandsome return for his kindness, and would on the spot have referred the author and the book to the tribunal of the Holy Office, had he not been restrained by the importu nities of the embassador Niccolini, and his fear of offending the Grand-duke of Tuscany. "Galileo," said Urban, "has not acted with out deliberation, has not sinned through ignorance; he was perfectly well aware of the difficulties of the case, for I myself have made them clear to him." These expressions of dissatisfaction on the part of the sovereign pontiff would seem to show that, in the interviews of which we have spoken, the two friends had touched on the delicate question of the earth's motion, and that, by a process of self-illusion quite natural under the circumstances, each had supposed he had convinced the other. The pope was angry at Galileo, as at one in whom he had for a long time mistakenly reposed confidence—as though a fraud had been practised upon him; this feeling, which had broken the bond of their old friendship, explains the harshness with which Urban treated the friend of his youth. Nor had Galileo been less mistaken with regard to the disposition of the pope's mind. He flattered himself that he should find in him an indulgent judge of his astronomical theories, while in point of fact he was wounding Urban in his most sacred convictions. Had he known that the pope was so opposed to the system of Copernicus, doubtless he never would have braved the wrath of one whose power was unlimited, or affronted a tribunal from which there was no appeal.

On receipt of the "Dialogues," Urban instructed a commission to examine the book and report to him. As soon as the report came into his hands, he commanded the inquisitor at Florence to communicate to Galileo a formal summons to appear in October before the commissary-general of the Holy Office in Rome. Galileo, then seventy years of age, and suffering from hernia, asked the authorities to take into consideration his age and his malady, and to dispense him from the journey. The Grand-duke of Tuscany interceded for him. But

Urban would listen to nothing; fearing lest he should be deceived, as he believed he had been before, he would permit no delay. He would not even believe the testimony of three physicians who attested the reality of Galileo's malady; he sent the inquisitor in person to him, with orders to arrest and bring him in irons to Rome, if he was found to be in a condition to bear the journey. Poor Galileo had taken to his bed, and, as was said by one of his friends, "he was more in danger of going to the other world than to Rome." He was not in a condition to be removed until January, 1633. The good offices of the Grand-duke of Tuscany attended him to the presence of his judges, and there the friendship of Niccolini accompanied him—weak succors these in the face of such powerful adversaries. At first the embassador's palace was appointed as his place of confinement, and he was commanded not to leave it; he went out only in order to submit to the interrogatories proposed to him by the Holy Office.

On the 12th of April he was interrogated for the first time. To begin with, he was asked if he remembered what took place in 1616, when he had to appear before Cardinal Bellarmin and the commissary-general of the Holy Office. Galileo admitted having heard it declared on that day that the system of Copernicus could not be maintained or defended, as being contrary to the Holy Scriptures. "It may be," he added, "that at the same time I myself was forbidden to maintain or defend that opinion, but I do not recollect, it is now so long ago." Whatever may be the interest now taken in a case so bound up with the question of the freedom of thought, it is not easy to believe with Berti that Galileo replied to this first interrogatory with entirely good faith. When a prohibition is issued in terms so formal as those we have given, upon so definite a point, neither the form nor the substance is ever forgotten. Ambiguity was out of the question after Bellarmin's warning, and still more after the solemn injunction of the commissary-general. Domenico Berti is in error with regard to the psychological conditions of memory where he says that it must have been easier for Galileo to recollect the conciliatory words of Cardinal Bellarmin than the threats of the commissary-general. On the contrary, what strikes one most under such circumstances, what impresses itself deepest in the memory, is the threats. How could any one forget words so simple, so clear, so menacing, as these: "You are forbidden to maintain this opinion, to teach or to defend it, whether by writing or by word of mouth, or in any other manner whatsoever, else the Holy Office will take information against you!" These last words in particular must have buried themselves like an arrow in the memory of Galileo, nevermore to come out. He knew all too well what he had to fear from the Inquisition ever to forget on what conditions that tribunal agreed to take no further cognizance of him. The silence he kept in public for sixteen years upon the forbidden subject, and even the care he took in his "Dialogues" to give to his thoughts an in-

offensive turn, might serve as evidence of the faithfulness of his memory.

The fact is, that the reason of Galileo's taking up his pen again to treat a forbidden subject was not that he had forgotten the formal prohibition. He might have made answer, with great frankness, that, though he had been ordered to hold his peace, yet he had not been convinced, and that, after so many years of silence, the need of proclaiming the truth had more power over him than the fear of disobeying. But it was not for a mind so subtile, nor for a character so wary as that of Galileo, to be tied down to a categorical declaration, and so to shut every portal of escape. He chose rather to use evasions with his judges, to plead extenuating circumstances, to produce the impression that he might have misunderstood, but that he had not acted with evil intent and with his eyes open. Even while undergoing the first interrogatory, he was still in hopes of finding in the sovereign pontiff some remnant of friendship, or, at least, of good-will; and this was another reason why he made an evasive reply, and did not compromise himself by an explicit admission of his offenses. He appears to have believed, at this first session, that it would be possible for him to have a private interview with the holy father. Being questioned as to what had been said to him by Cardinal Bellarmin in 1616, he replied that some of the details of their conversation he could intrust only to the ear of the sovereign pontiff. This plainly was a request for an interview with Urban. His judges seemed not to understand him, or, if they carried his words to the holy father, they obtained from him no favorable answer; but, in the course of the trial, it became evident that Galileo could expect neither indulgence nor commiseration from his old friend.

All of Galileo's answers at the first interrogatory present the same character of ambiguity. On being asked whether, before he begged of Father Riccardi license to print his "Dialogues," he had informed the master of the Sacred Palace of his having previously been forbidden to treat certain subjects, his reply was that he had not mentioned that to Father Riccardi, "for he did not think it necessary to do so, having no scruples, nor having supported or defended in his book the opinion of the earth's motion and the stability of the sun." It is not altogether certain that, by thus altering the truth, Galileo chose the best line of defense; probably a little more of frankness would have served him better. He was simply trifling with his judges and taking them for fools, when he tried to make them believe that, in his "Dialogues," his purpose had been to demonstrate the "weakness and insufficiency" of Copernicus's arguments. The disguises in which the author clothes his thoughts fail to deceive the thoughtful reader. Throughout the work, the defender of Ptolemy's theory, Simplicio (in whom it has been wrongfully supposed that some of the traits of Urban VIII. may be found), is overthrown by his opponents'

arguments, and made an object of ridicule by their irony. Surely, it was imprudent on the part of Galileo to deny the evidence, thus giving to his defense the appearance of double-dealing.

Nor did the resort to this course deceive any one. The three judges who had questioned him unanimously declared that in his book he had contravened the injunctions of Cardinal Bellarmin, and the decree of the Congregation of the Index. Two of them added that he was gravely suspect of adhering to the doctrine of Copernicus. After the close of his first interrogatory, he was removed to the palace of the Holy Office, and there he occupied a chamber in the sleeping-apartments of the wardens, with an express prohibition of going out without leave. Here he had long and frequent interviews with Father Vincenzo Macolano, commissary of the Holy Office, an educated man of kindly disposition, and a friend of the grand-duke and of the Tuscan embassador; Father Macolano took it upon himself to warn Galileo of the dangers of the situation, and to aid him with his counsels. First of all, he induced Galileo to submit without reserve, to admit his offenses, and to repent. "I made his error patent to him," wrote the father commissary, at the close of one of their interviews; "he clearly saw that he had made a mistake, that in his book he had gone too far, and he expressed to me his regret in words full of feeling, as though he drew comfort from the knowledge of his error, and was thinking of confessing it judicially; he only asked of me a little time to consider how he might best word his confession." Father Macolano then looked for a speedy ending of the trial, and a less severe sentence. "When once we have Galileo's confession," said he, "the reputation of the tribunal will be safe, and the accused can be treated with indulgence." Evidently he expected that the case would not be carried beyond the first stage of inquisition, and that it would terminate by a special form of interrogatory, known as the "interrogatory with regard to the intention."

If things were pushed further than the commissary of the Holy Office either wished or expected, the blame does not rest with the accused, who, once warned, immediately resolved to submit. On being interrogated again on the 30th of April, Galileo confessed that, without meaning it, he had presented too forcibly the arguments in favor of the system of Copernicus, his intention all the while being to refute them, and that thus he might have led the public into error. He declared that he was "ready to refute the opinion of Copernicus by all the most efficacious methods that God might place within his power." These words, no doubt dictated to him by the humanity of the father commissary, had the effect of procuring for him some measure of liberty. That very evening he was sent back to the palace of the Tuscan embassador, so that there he might receive such care as the state of his health required.

We must not forget that to the humiliation of repudiating his

most cherished opinions, of belying his own thoughts, and of seeing himself treated as a criminal after he had, by his labors, done honor to his country and to mankind, were added physical sufferings of the most grievous kind. It is impossible to read without emotion the appeal he addressed to his judges at the end of his written defense: "It remains for me to urge one final consideration, viz., the pitiable state of bodily indisposition to which I have been reduced by incessant mental agony during ten whole months, together with the hardships of a long and toilsome journey, in the most inclement weather, at the age of threescore years and nine. . . . I confide in the mercy and goodness of the most eminent seigniors who are my judges, and I hope that if, in the integrity of their justice, they think that so great sufferings lack anything to make them equal to the punishment that my offenses deserve, they will be pleased, at my entreaty, to remit the difference in consideration of the failing strength of my old age, which I humbly commend to them."

Among the hitherto unpublished documents contained in Berti's work there is one that is of the highest importance. This is a summary of the case, giving an enumeration not only of what was decreed but also of what was done. After reading a text so clear and so unambiguous on all points save one, while on that one it agrees perfectly with other authentic documents, we no longer find ground for supposing it was only on paper that Galileo was threatened with the torture, and forced to make abjuration. A decree of the pope, dated June 16th, ordains that instead of a simple "examination as to intention," such as the commissary of the Holy Office had expected, an interrogatory[1] should be had with the threat of torture, if the accused could stand it; he is ordered to make abjuration, and condemned to imprisonment according to the good pleasure of the congregation. This decree was not, as has been supposed, a simple declaration designed to sustain the reputation of the tribunal for severity, while the culprit was treated leniently; on the contrary, it was executed literally, as is shown by the agreement of the documents concerning this portion of the trial.

On being interrogated for the last time on the 21st of June, Galileo was ordered to state whether he then held or ever had held the opinion that the sun is the centre of the world and that the earth moves. He humbly replied that ever since the decree of the Congregation of the Index, in 1616, he had always held and still did hold the opinion of Ptolemy to be "most true and unquestionable." This reply not appearing to be satisfactory, the father commissary insisted on knowing the truth, and wound up by declaring that, if the whole truth were not stated, recourse would be had to torture. "I am here in

[1] AUTHOR'S NOTE.—We interpret three obscure words in the pontifical decree, *ac si sustinuerit*, in the sense given to them by Berti. Th. Henri Martin gives a different translation, not without good reasons. The matter is one that will bear discussion. It is still undecided, even after the publication of the documents.

order to obey," replied Galileo, with some show of terror. The text of the sentence shows that he was treated more rigorously yet. "Inasmuch as it appears to us that you have not told the whole truth as touching your intention, we have deemed it necessary to resort to the *examen rigorosum*." Now, in the language of the Inquisition, *examen rigorosum* means just the torture and that alone: it is the law-term approved by jurists and regularly employed in sentences which condemn the accused to the cruel punishment of the *strappado*. "In case the accused," say the treatises on inquisitorial law, "does not clear himself of the charges, recourse is to be had to the *examen rigorosum*, torture having been devised to supply the want of witnesses." In two manuscripts of the first half of the seventeenth century, both of them relating to the forms of procedure of the Holy Office, the expression *examen rigorosum* is pointed out as the formula to be employed by judges in ordering the application of torture.

From the text of the sentence, from the pontifical decree already quoted, and from the summary of the acts of the trial, we might infer that Galileo was actually subjected to torture, if among the documents we found the official record (*procès-verbal*) of the *examen rigorosum*, as we find the official record of the previous examinations. The rule of the Inquisition was ever the same: the notary or registrar of the Holy Office was present at all interrogatories, and took down carefully the words of the sufferer; all the details of the *examen rigorosum* were recorded in a register, from the first intimation to the accused that he was to be taken to the place of punishment, down to the moment when he was released from the torture. On looking over the records of these dread sessions, we find all the words spoken by the sufferer while his clothes are being taken off and while he is being tied to the instrument of torture; all the replies he makes to his judges, all his pleas; every movement he makes is noted with cold precision, nay, even his sighs, his groans, while under the torture. "He was hoisted by the rope," calmly writes the notary, "and while suspended he would cry out in a loud voice, 'O Lord God, have pity! O Our Lady, help me!' repeating these words again and again. Then he was silent, and having for a little while thus held his peace, he began again to cry out, 'O God, O God!'"

If Galileo had been subjected to this mode of trial, the *procès-verbal* of the proceedings would certainly have been preserved along with the other records of the case. But, then, might not the *examen rigorosum* have taken place in the absence of the registrar; or might not the registrar, though he was present, have omitted to make a record? Both of these suppositions appear to be equally inadmissible, for they are in flat contradiction to all the precedents and all the rules of the tribunal. Neither can we suppose that the agent of the Holy Office suppressed the *procès-verbal* of the torture in order that both he and his principals might escape the indignation of posterity.

This were gratuitously to transform an obscure, an irresponsible personage into an humanitarian philosopher who is ages ahead of the thought of his time and who purposely destroys a sorrowful page of history. The most probable account of what took place would be this: According to all the treatises on inquisitorial law, the commissary was authorized not to inflict torture on aged men, or on persons suffering from disease, who might die under the punishment. The advanced age of Galileo, and his infirmities, aggravated as they were by so much mental suffering, naturally placed him in the category of culprits who were not subjected to torture. If he was spared that dreadful infliction, Berti gives all the credit to the humanity of the father commissary; he even appears to think that, but for the kindly intervention of Father Macolano, the sovereign pontiff and the Congregation of the Holy Office would have given over Galileo to the executioner.

Let us be more fair. It would be a libel on Urban VIII. to represent him as thirsting for the blood and pleased with the sufferings of his old friend. The pontifical decree of June 16th has this important proviso regarding the employment of torture, that it should not be used unless the accused could endure it. When he expressed himself thus, the sovereign pontiff was perfectly well aware that Galileo could not stand such a trial, and he consented beforehand, without needing to be entreated by the commissary, to the omission of the torture. What, indeed, would have been the use of such extreme rigor? Urban did not desire the death of the culprit; he wanted to make certain that Galileo would never more speak or write about the question of the earth's motion; and it was in order to so strike him with terror as to insure his silence that of all the agonies of the trial he saved him only from the last—the only one that would have been of no use. The pope was not so cruel as Berti thinks, but neither did he give any sign of that compassion and indulgence toward the accused with which he is too often credited. This point is worth repeating, inasmuch as it is the clearest result of Berti's publication: the various phases of the trial of Galileo were not arranged with a view to theatrical effect, and to make an outward show of great severity, so as to intimidate the adherents of Galileo's doctrine, while, behind the scenes, the culprit was treated with kindness. The threat of torture, the abjuration, the sequestration, were realities, and not, as has been supposed, simply monitions addressed to overbold men of science. At first, the court of Rome did not concern itself so much about impressing the imagination of the public as about striking Galileo. Here was a rebellious subject who had once before been treated with the greatest lenience, but who repaid the indulgence of the Holy Office with the transparent irony of his "Dialogues;" who had set snares for the person appointed to examine his manuscript; who, at his first interrogatory, had made sport of his judges, nay, perhaps of the sovereign pontiff himself: he must now be reduced to silence for

good and all, by conducting him, through a series of moral tortures, to the uttermost limits of terror.

At the same time the solemn form of his abjuration was calculated to prevent him from ever again inclining toward the Copernican doctrine. How could he embrace that doctrine again after he had openly pronounced it heretical, and promised, as he was compelled to do, to inform upon all persons suspected of this heresy? His judges, however, were not yet satisfied; he was feared even after his abjuration. He was confined at first in the palace of the Archbishop Piccolomini, at Siena, then at his own villa of Arcetri, near Florence, with leave to receive a few visits of relatives and friends, but on condition that several persons should never meet there to hold conversation. It was particularly feared that he would communicate with learned men abroad and in Italy. Father Castelli, his old pupil, in vain begged leave to see him, though he promised not to talk with him about the earth's motion. In order to protect all other Catholic countries against the contagion of his ideas, the pope dispatched to all apostolic nuncios and to all inquisitors copies of the sentence pronounced on Galileo, and of his act of abjuration. At Florence his chief disciples and friends, especially the professors of mathematics, were summoned by name to listen to the reading of these two documents.

In shutting the mouth of a writer so gifted, so full of resources, so admired by the public, it was hoped that an end was made of the doctrine of Copernicus—that dangerous doctrine which alarmed the theologians by displacing the centre of the universe, ousting the earth from its primacy and substituting the sun, and opening the way for hypotheses of the plurality of worlds and the end of creation. But the effort was vain. The theory of the earth's motion has survived all condemnations. It was not Galileo, as tradition would have it, that uttered the famous saying, "*Eppur si muove*," but the general voice of mankind who, after his death, thus proclaimed the undying truth of his belief.

Here we will stop. We would not weaken, by any comments of ours, the importance of the documents we have been examining. It is a fixed historical fact that in the beginning of the seventeenth century the Roman congregations, assuming to represent the Church, and not disavowed by her, made themselves the judges of a scientific question, and decided it in a way contrary to the conclusions of science. The splendor of Galileo's genius and the commiseration inspired by his sufferings impress upon this discussion a tragical and popular character; but the emotion produced by his cruel fate must not blind us to the gravity of the problem. The great question was whether, in countries that were then Catholic and destined so to remain, Science could free herself from the dominion of Faith. The trial of Galileo, so far from retarding this conclusion, as is commonly supposed, on the contrary made it inevitable and urgent. So soon as the

court of Rome saw how unwisely she had acted in deciding a question beyond her competence, thus laying herself open to the danger of being the next day convicted of error, it became her interest, no less than the interest of Science, to distinguish clearly between the two domains, Science and Faith. If, nowadays, she avoids entering into scientific controversies, it is because she has been taught by experience that a decision might compromise her. Her authority could hardly stand after a second edition of the sentence in which she once forbade the sun to stand still and the earth to revolve.

## DISTANCE AND DIMENSIONS OF THE SUN.

By Professor C. A. YOUNG,
of Dartmouth College.

THE problem of finding the distance of the sun is one of the most important and difficult presented by astronomy. Its importance lies in this, that this distance—the radius of the earth's orbit—is the base-line by means of which we measure every other celestial distance, excepting only that of the moon; so that error in this base propagates itself in all directions through all space, affecting with a corresponding proportion of falsehood every measured line—the distance of every star, the radius of every orbit, the diameter of every planet.

Our estimates of the masses of the heavenly bodies also depend upon a knowledge of the sun's distance from the earth. The quantity of matter in a star or planet is determined by calculations whose fundamental data include the distance between the investigated body and some other body whose motion is controlled or modified by it; and this distance generally enters into the computation by its cube, so that any error in it involves a more than threefold error in the resulting mass. An uncertainty of one per cent. in the sun's distance implies an uncertainty of more than three per cent. in every celestial mass and every cosmical force.

Error in this fundamental element propagates itself in time also, as well as in space and mass. That is to say, our calculations of the mutual effects of the planets upon each other's motions depend upon an accurate knowledge of their masses and distances. By these calculations, were our data perfect, we could predict for all futurity, or reproduce for any given epoch of the past, the configurations of the planets and the conditions of their orbits, and many interesting problems in geology and natural history seem to require for their solution just such determinations of the form and position of the earth's orbit in by-gone ages.

Now, the slightest error in the data, though hardly affecting the result for epochs near the present, leads to uncertainty which accumulates with extreme rapidity in the lapse of time; so that even the present uncertainty of the sun's distance, small as it is, renders precarious all conclusions from such computations when the period is extended more than a few hundred thousand years from the present time. If, for instance, we should find as the result of calculation with the received data that two millions of years ago the eccentricity of the earth's orbit was at a maximum, and the perihelion so placed that the sun was nearest during the northern winter (a condition of affairs which it is thought would produce a glacial epoch in the southern hemisphere), it might easily happen that our results would be exactly contrary to the truth, and that the state of affairs indicated did not occur within half a million years of the specified date—and all because in our calculation the sun's distance, or solar parallax by which it is measured, was assumed half of one per cent. too great or too small. In fact, this solar parallax enters into almost every kind of astronomical computations, from those which deal with stellar systems and the constitution of the universe to those which have for their object nothing higher than the prediction of the moon's place as a means of finding the longitude at sea.

Of course, it hardly need be said that its determination is the first step to any knowledge of the dimensions and constitution of the sun itself.

This parallax of the sun is simply *the angular semi-diameter of the earth as seen from the sun;* or, it may be defined in another way as the angle between the direction of the sun ideally observed from the centre of the earth, and its actual direction as seen from a station where it is just rising above the horizon.

We know with great accuracy the dimensions of the earth. Its mean equatorial radius, according to the latest and most reliable determination (agreeing, however, very closely with previous ones), is 3962.720 English miles [6377.323 kilometres], and the error can hardly amount to more than $\frac{1}{100000}$ of the whole—perhaps, 200 feet one way or the other. Accordingly, if we know how large the earth looks from any point, or, to speak technically, if we know the parallax of the point, its distance can at once be found by a very easy calculation: it equals simply [206,265[1] × the radius of the earth] ÷ [the parallax in seconds of arc].

Now, in the case of the sun it is very difficult to find the parallax with sufficient precision on account of its smallness—it is less than 9″, almost certainly between 8.8″ and 8.9″. But this tenth of a second

[1] This number 206,265 is the length of the radius of a circle expressed in seconds of its circumference. A ball one foot in actual diameter would have an apparent diameter of one second at a distance of 206,265 feet, or a little more than 39 miles. If its apparent diameter were 10″, its distance would, of course, be only $\frac{1}{10}$ as great.

of doubtfulness is more than $\frac{1}{100}$ of the whole, although it is no more than the angle subtended by a single hair at a distance of nearly 800 feet. If we call the parallax 8.86″, which is probably very near the truth, the distance of the sun will come out 92,254,000 miles, while a variation of $\frac{1}{20}$ of a second either way will change it nearly half a million of miles.

When a surveyor has to find the distance of an inaccessible object, he lays off a convenient base-line, and from its extremities observes the directions of the object, considering himself very unfortunate if he cannot get a base whose length is at least $\frac{1}{10}$ of the distance to be measured. But the whole diameter of the earth is less than $\frac{1}{11000}$ of the distance of the sun, and the astronomer is in the predicament of a surveyor who, having to measure the distance of an object ten miles off, finds himself restricted to a base of less than five feet, and herein lies the difficulty of the problem.

Of course, it would be hopeless to attempt this problem by direct observations, such as answer perfectly in the case of the moon, whose distance is only thirty times the earth's diameter. In her case, observations taken from stations widely separated in latitude, like Berlin and the Cape of Good Hope, or Washington and Santiago, determine her parallax and distance with very satisfactory precision; but if observations of the same accuracy could be made upon the sun (which is not the case, since its heat disturbs the adjustments of an instrument), they would only show the parallax to be somewhere between 8″ and 10″, its distance between 126,000,000 and 82,000,000 miles.

Astronomers, therefore, have been driven to employ indirect methods based on various principles: some on observations of the nearer planets, some on calculations founded upon the irregularities—the so-called perturbations—of lunar and planetary movements, and some upon observations of the velocity of light. Indeed, before the Christian era, Aristarchus of Samos had devised a method so ingenious and pretty in theory that it really deserved success, and would have attained it were the necessary observations susceptible of sufficient accuracy. Hipparchus also devised another founded on observations of lunar eclipses, which also failed for much the same reasons as the plan of Aristarchus.

The idea of Aristarchus was to observe carefully the number of hours between new moon and the first quarter, and also between the quarter and the full. The first interval should be shorter than the second, and the difference would determine how many times the distance of the sun from the earth exceeds that of the moon, as will be clear from the accompanying figure. The moon reaches its quarter, or appears as a half-moon, when it arrives at the point Q, where the lines drawn from it to the sun and earth are perpendicular to each other. Since the angle H E Q = E S Q, it will follow that H Q is the same fraction of H E as Q E is of E S; so that, if H Q can be found,

we shall at once have the ratio of Q E and E S. Aristarchus thought he had ascertained that the first quarter of the month (from N to Q) was about 12 hours shorter than the second, from which he computed the sun to be about 19 times as distant as the moon. The difficulty

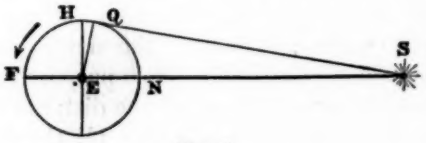


Fig. 1.

lies in the impossibility of determining the precise moment when the disk of the moon is an exact semicircle. The real difference between the first and second quarters is really not quite 36 minutes, and the sun's distance is about 400 times the moon's.

The different methods upon which our present knowledge of the sun's distance depends may be classified as follows:

1. Observations upon the planet Mars near opposition, in two distinct ways:
   (*a*) Observations of the planet's declination made from stations widely separated in latitude.
   (*b*) Observations from a single station of the planet's right ascension when near the eastern and western horizons — known as Flamsteed's method.
2. Observations of Venus at or near inferior conjunctions:
   (*a*) Observations of her distance from small stars measured at stations widely different in latitude.
   (*b*) Observations of the transits of the planet: 1. By noting the *duration* of the transit at widely-separated stations; 2. By noting the true Greenwich time of contact of the planet with the sun's limb; 3. By measuring the distance of the planet from the sun's limb with suitable micrometric apparatus; 4. By photographing the transit, and subsequently measuring the pictures.
3. By observing the oppositions of the nearer asteroids in the same manner as those of Mars.
4. By means of the so-called parallactic inequality of the moon.
5. By means of the monthly equation of the sun's motion.
6. By means of the perturbations of the planets, which furnish us the means of computing the ratios between the *masses* of the planets and the sun, and consequently their distances—known as Leverrier's method.
7. By measuring the velocity of light, and combining the result (*a*) with equation of light between the earth and sun, and (*b*) with the constant of aberration.

Our scope and limits do not, of course, require or allow any exhaustive discussion of these different methods and their results, but some of them will repay a few moments' consideration:

The first three methods are all based upon the same general idea, that of finding the actual distance of one of the nearer planets by ob-

serving its displacement in the sky as seen from remote points on the earth. The *relative* distances of the planets are easily found in several different ways,[1] and are known with very great accuracy—the possible error hardly reaching the ten-thousandth in even the most unfavorable cases. In other words, we are able to draw for any moment an exceedingly accurate map of the solar system—the only question being as to the scale. Of course, the determination of any line in the map will fix this scale; and for this purpose one line is as good as another, so that the measurement of the distance from the earth to the planet Mars, for instance, will settle all the dimensions of the system.

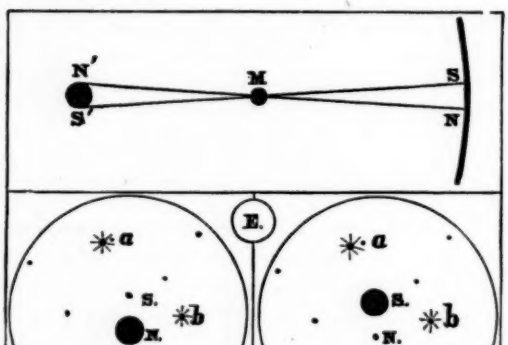


FIG. 2.

The figure illustrates the method of observation. Suppose two observers, situated one near the north pole of the earth, the other near the south. Looking at the planet, the northern observer will see it at *N* (in the upper figure), while the other will see it at *S*, farther north in the sky. If the northern observer sees it as at *A* (in the lower part of the figure), the southern will at the same time see it as at *B*; and, by measuring carefully at each station the apparent distance of the planet from several of the little stars (*a*, *b*, *c*) which appear in the field of view, the amount of the displacement can be accurately ascertained. The figure is drawn to scale. The circle *E* being taken to represent the size of the earth as seen from Mars when nearest us, the black disk represents the apparent size of the planet on the same scale, and the distance between the points *N* and *S*, in either figure *A* or *B*, represents, on the same scale also, the displacement which would

[1] One method of determining the relative distances of a planet and the sun from each other and from the earth is the following, known since the days of Hipparchus: First, observe the date when the planet comes to its opposition—i. e., when sun, earth, and planet, are in line, as in the figure, where the planet and earth are represented by *M* and *E*. Next, after a known number of days, say one hundred, when the planet has advanced

be produced in the planet's position by a transference of the observer from Washington to Santiago, or *vice versa*.

The first modern attempt to determine the sun's parallax was made by this method in 1670, when the French Academy of Sciences sent Richer to Cayenne to observe the opposition of Mars, while Cassini (who proposed the expedition), Roemer, and Picard, observed it from different stations in France. When the results came to be compared, however, it was found that the planet's displacement was imperceptible by their existing means of observation: from this they inferred that the planet's parallax could not exceed half a minute of arc, and that the sun's could not be more than 10″.

In 1752 Lacaille at the Cape of Good Hope made similar observations, and their comparison with corresponding observations in Europe showed that instruments had so far improved as to make the displacement quite sensible. He fixed the sun's parallax at 10″, corresponding to a distance of about 82,000,000 miles.

In more recent times the method has been frequently applied, and with results on the whole satisfactory. In 1849–'52 Lieutenant Gilliss was sent by the United States Government to Santiago, in Chili, to observe both Mars and Venus in connection with northern observatories. In 1862 a still more extended campaign was organized, in which a great number of observatories in both hemispheres participated. Prof. Newcomb's careful reduction of the work puts the resulting parallax at 8.855″. The method can be used to the best advantage, of course, when at the time of opposition the planet is near its perihelion and the earth near its aphelion; these favorable oppositions occur about once in fifteen years, and the one which is next to occur, in September, 1877, is so exceptionally advantageous that already somewhat extensive preparations are on foot to secure its careful and general observation.

to $M'$ and the earth to $E,'$ observe the planet's elongation from the sun, i. e., the angle $M' E' S$. Now, since we know the periodic times of both the earth and planet, we shall know both the angle $M S M'$ moved over by the planet in one hundred days, and also $E S E'$ described in the same time by the earth, the difference is $M' S E,'$ called by some

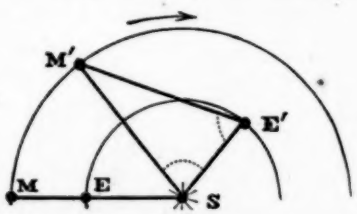


FIG. 3.

writers the synodic angle. We have, therefore, in the triangle $M' S E,'$ the angle at $E'$ measured, and the angle $M' S E'$ known as stated above; this of course gives the third angle at $M,'$ and hence we know the *shape* of the triangle, and by the ordinary processes of trigonometry can find the relative values of its three sides.

In observations of this sort upon Mars or the asteroids, the position and displacement of the planet, as seen from different stations, are determined by comparing it with neighboring stars. When Venus, however, is nearest us, she can be observed only by day, so that in her case star comparisons are as a general thing out of the question. But occasionally at her inferior conjunction she passes directly across the disk of the sun, and her parallactic displacement from different stations can then be determined by making any such observations as will enable the computer to ascertain accurately her apparent distance and direction from the sun's centre at some given moment. Gregory in 1663 first pointed out the utility of such observations for ascertaining the parallax, but it was not until some fifteen years later that the attention of astronomers was secured to the subject by Halley, who discussed the matter thoroughly, and showed how the problem might be solved with accuracy by observations such as were entirely practicable even by the instruments and with the knowledge then at command. In 1761 and 1769 two transits occurred which were observed in all accessible quarters of the globe by expeditions sent out by the different governments. From different sets of these observations variously combined by different computers, values of the solar parallax were obtained ranging all the way from 7.5″ to 9.2″. A general discussion of all the materials afforded by the two transits was first made by Encke in 1822, and he obtained, as the most probable result, the value 8.″5776, which from that time for more than thirty years was accepted by all astronomers as the best attainable approximation to the truth. In order to harmonize the results, however, he thought himself obliged to reject the observations of several stations. In 1854 Hansen, in publishing some of his results respecting the motion of the moon, announced that Encke's value of the solar parallax could not be reconciled with his investigations; within the next six or seven years several independent researches by other astronomers confirmed his conclusions, and the most recent recomputations show that the observations rejected by Encke are as trustworthy as any, and that the errors of observation were so considerable in 1769 that nothing more can be fairly deduced from that transit than that the solar parallax is probably somewhere between 8.7″ and 8.9″.

The method of observation then used consisted simply in noting the moment when the limb of the planet came in contact with that of the sun—an observation which is attended with much more difficulty and uncertainty than would at first be supposed. The difficulties depend in part upon the imperfections of optical instruments and the human eye, partly upon the essential nature of light, leading to what is known as diffraction, and partly upon the action of the planet's atmosphere. The two first named causes produce what is called irradiation, and operate to make the apparent diameter of the planet, as seen on the solar disk, smaller than it really is—smaller, too, by an

amount which varies with the size of the telescope, the perfection of its lenses, and the tint and brightness of the sun's image. The edge of the planet's image is also rendered slightly hazy and indistinct.

The planet's atmosphere also causes its disk to be surrounded by a narrow ring of light, which becomes visible long before the planet touches the sun, and at the moment of internal contact produces an appearance of which the accompanying figure is intended to give an

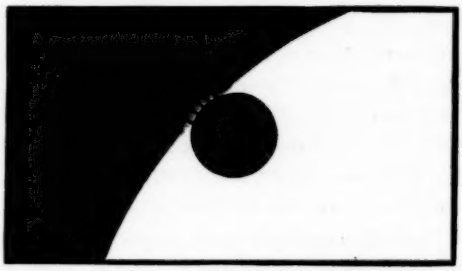

FIG. 4.

idea though on an exaggerated scale. The planet moves so slowly as to occupy more than twenty minutes in crossing the sun's limb; so that, even if the planet's edge were perfectly sharp and definite, and the sun's limb undistorted, it would be very difficult to determine the precise second at which contact occurs; but as things are, observers, with precisely similar telescopes, and side by side, often differ from each other five or six seconds; and where the telescopes are not similar the differences and uncertainties are much greater. The contact observations of the last transit in 1874 do not appear to be much more accordant than those of 1769, notwithstanding the great improvement in telescopes; and astronomers at present are pretty much agreed that such observations can be of little value in removing the remaining uncertainty of the solar parallax, and are disposed to put more reliance upon the micrometric and photographic methods, which are free from these peculiar difficulties, though of course beset with others; which, however, it is hoped will prove less formidable.

The micrometric method requires the use of a peculiar instrument known as the heliometer, an instrument common only in Germany, and requiring much skill and practice in its use in order to obtain with it accurate measures. At the late transit a single English party, two or three of the Russian parties, and all five of the German, were equipped with these instruments, and at some of the stations extensive series of measures were made. None of the results, however, have appeared as yet, so that it is impossible to say how greatly, if at all, this method will have the advantage in precision over the contact observations.

The Americans and French placed their main reliance upon the photographic method, while the English and Germans also provided

for its use to a certain extent. The great advantage of this method is that it makes it possible to perform the necessary measurements, upon whose accuracy everything depends, at leisure after the transit, without hurry, and with all possible precautions. The field-work consists merely in obtaining as many and as good pictures as possible. The only objection to the method lies in the difficulty of obtaining good pictures, i. e., pictures free from distortion, and so distinct and sharp as to bear high magnifying power in the microscopic apparatus used for their measurement. It is necessary also that the exact scale of the pictures, or the number of seconds of arc to the linear inch, be known, as well as the precise Greenwich time at which each picture is taken, and it is also extremely desirable that the *orientation* of the picture should be accurately determined, that is, the north and south, east and west points of the solar image on the finished plate. There has been a good deal of anxiety lest the image, however accurate and sharp when first produced, should alter in course of time through the contraction of the collodion film on the glass plate, but the experiments of Rutherfurd, Huggins, and Paschen, seem to show that this danger is imaginary; that if a plate is properly prepared the collodion film never creeps at all, but remains firmly attached to the glass. It requires but a very trifling amount of distortion or inaccuracy of the image to render it useless. The uncertainty in our present knowledge of the sun's parallax is so small that it would only involve an error of about one-quarter of a second in the calculated position of Venus on the sun's disk as seen from any station at any given time during the transit, and this would be about $\frac{1}{2000}$ of an inch on a four-inch picture of the sun. Unless, then, the picture is so distinct and free from distortion that the relative positions of Venus and the sun's centre can be determined from it within $\frac{1}{2000}$ of an inch, it is worthless as a means of correcting the received determination of the parallax.

But it is to be noted that any mere enlargement or diminution of the diameter of sun or planet will do no harm, provided it is alike all around the circumference of the disk, since the measurement is not from the edge of Venus to the edge of the sun, but between their *centres*. Photographic determinations of *contact*, on the contrary (such as Janssen and some of the English parties attempted by a peculiar and complicated apparatus), are affected with all the uncertainties of the old-fashioned observations of the eye alone, and with others in addition; so that, astronomically considered, they are entirely worthless, although interesting from a chemical and physical point of view.

Two essentially different lines of proceeding were adopted, at the last transit, in the photographic observations. The English and Germans attached a camera to the eye-end of an ordinary telescope, which was pointed directly at the sun; the image formed at the focus of the telescope was enlarged to the proper size by a combination of lenses

in the camera; and a small plate of glass ruled with squares was placed at the focus of the telescope and photographed with the sun's image, furnishing a set of reference-lines, which give the means of detecting and allowing for any distortion caused by the enlarging lenses.

The Americans and French, on the other hand, preferred to make the picture of full size, without the intervention of any enlarging lens: as this requires an object-glass with a focal length of thirty or forty feet, which could not be easily pointed at the sun, a plan proposed first, I believe, by M. Laussedat, but also independently by our own Prof. Winlock, was adopted. The telescope is placed horizontal, and the rays are reflected into the object-glass by a plane mirror suitably mounted. The French used mirrors of silvered glass, and took their pictures (about two and a half inches in diameter) by the old daguerreotype process on silvered plates of copper, in order to avoid the risk of collodion-contraction. With the silvered mirror the time of exposure is so short that no clock-work is required. The Americans used *unsilvered* mirrors, in order to avoid any distorting action of the sun's rays upon the form of the mirror. This, of course, made the light feebler, and the time of exposure longer, so that a clock-work movement of the mirror was needed to keep the image from changing its place on the plate during the exposure, which, however, never exceeded half a second. The American pictures were taken by the ordinary wet process on glass, and were about four inches in diameter. Just in front of the sensitive plate, at a distance of about one-eighth of an inch, was placed a reticle, or a plate of glass ruled in squares, and between this and the collodion-plate hung a fine silver wire suspending a plumb-bob. Thus the finished negative was marked into squares, and also bore the image of the plumb-line, which, of course, indicated precisely the direction of the vertical. The Americans also placed the photographic telescope exactly in line with a meridian instrument, and so determined, with the extremest precision, the direction in which it was pointed. Knowing this, and the time at which any picture was taken, it becomes possible, with the help of the plumb-line image, to determine precisely the orientation of the picture —an advantage possessed by the American pictures alone, and making their value nearly twice as great as otherwise it would have been.

The following figure is a representation of one of the American photographs reduced about one-half. $V$ is the image of Venus, which on the actual plate is about one-seventh of an inch in diameter; $a\,a'$ is the image of the plumb-line. The centre of the reticle is marked by the little cross, and the word "China," written on the reticle-plate with a diamond—and, of course, copied on the photograph—indicates that it is one of the Peking pictures. Its number in the series is given in the right-hand upper corner. About 90 such pictures were obtained at Peking during the transit, and about 350 at all the eight American stations, the work being much interfered with by unfavorable weather

at most of them. If we add those obtained by the French, Germans, and English, the total number available reaches nearly 1,200, according to the best estimates.

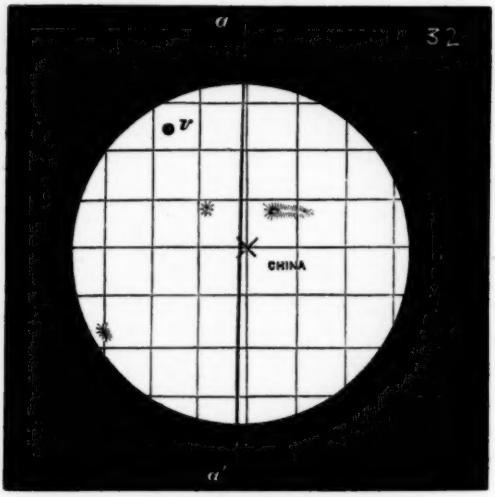


FIG. 5.

After the pictures are made and safely brought home, they have next to be measured—i. e., the distance (and in the American pictures the *direction* also) between the centre of Venus and the centre of the sun must be determined in each picture. This is an exceedingly delicate and tedious operation, rendered more difficult by the fact that the image of the sun is never truly circular; but, even supposing the instrument to be perfect in all its adjustments, is somewhat distorted by the effect of atmospheric refraction; so that the true position of the sun's centre with reference to the squares of the reticle is determined only by an intricate calculation from measurements made with a microscopic apparatus on a great number of points suitably chosen on the circumference of the image. The final result of the measurement would come out something in this form: Peking, No. 32, time, $14^h\ 08'\ 20.2''$ (Greenwich mean time); Venus north of sun's centre, $735.32''$; east of centre, $441.63''$; distance from centre of sun, $857.75''$. (The numbers given are only imaginary.) It is this process of measurement which has required so long a time since the transit, and is not yet completed. When it is finished, and the results published in the form indicated, then will come the work of combining all the data thus obtained at all the stations, and from them deducing the true value of the solar parallax. Since, however, another transit is to occur so soon (in 1882), it is not unlikely that astronomers may defer the final grand combination until the observations of that transit also are ready to be included. It is very confidently hoped by most of those who have studied the subject that the remaining uncertainty in the

sun's distance will be greatly reduced as the result of this work; and yet there are some grounds for anxiety lest the photographic data prove as intractable and inconsistent as those derived from contact-observations. Time only can positively settle the question.

One of the best methods of determining the solar parallax is based upon the careful observation of the motions of the moon. It will be recollected that the first suspicion as to the correctness of the then received distance of the sun was raised in 1854 by Hansen's announcement that the moon's parallactic inequality led to a smaller value than that deduced from the transit of Venus—a conclusion corroborated by Leverrier four years later. It seems at first sight strange, but it is true, as Laplace long since pointed out, that the skillful astronomer, by merely watching the movements of our satellite, and without leaving his observatory, can obtain the solution of problems which, attacked by other methods, require tedious and expensive expeditions to remote corners of the earth. Our scope and object do not require us to enter into detail respecting this lunar method of finding the sun's parallax; it must suffice to say that the disturbing action of the sun makes the interval from new moon to full a little longer than that from full to new; and this difference depends upon the *ratio between the diameter of the moon's orbit and the distance of the sun* in such a manner that, if the inequality is accurately observed, the ratio can be calculated. Since we know the distance of the moon, this will give that of the sun. The results obtained in this way, according to the most recent investigations, fix the solar parallax between 8.83″ and 8.92″.

There is still another lunar method, mentioned in the synopsis; but its results are much less reliable—subject, that is, to a much larger probable error, though not at all contradictory to those just given.

But the method by which ultimately we shall obtain the most accurate determination of the dimensions of our system is that proposed by Leverrier, making use of the secular perturbations produced by the earth upon her neighboring planets, especially in causing the motions of their nodes and perihelia. These motions are very slow, but *continuous;* and hence, as time goes on, they will become known with ever-increasing accuracy. If they were known with absolute *precision, they would enable us to compute, with absolute precision also, the ratio between the masses of the sun and earth*, and from this ratio we can calculate[1] the distance of the sun by either of two or three different methods.

[1] One method of proceeding is as follows: Let M be the mass of the sun, and m that of the earth; let R be the distance of the sun from the earth, and r that of the moon; finally, let T be the number of days in a sidereal year, and t the number in a sidereal month. Then, by elementary astronomy,

$$M : m = \frac{R^3}{T^2} : \frac{r^3}{t^2}; \text{ whence } R^3 = r^3 \left(\frac{T^2}{t^2}\right) \left(\frac{M}{m}\right);$$

or, in words, *the cube of the sun's distance equals the cube of the moon's distance, multiplied*

As matters stand at present, the majority of astronomers would probably consider that these secular perturbations are not yet known with an exactness sufficient to render this method superior to the others that have been named—perhaps as yet not even their rival. Leverrier, on the other hand, himself puts such confidence in it that he declined to sanction or coöperate in the operations for observing the recent transit of Venus, considering all labor and expense in that direction as merely so much waste.

But, however the case may be now, there is no question that as time goes on, and our knowledge of the planetary motions becomes more minutely precise, this method will become continually and cumulatively more exact, until finally, and not many centuries hence, it will supersede all the others that have been described. The parallax of the sun, determined by Leverrier in this method, in 1872, comes out 8.86″.

The last of the methods mentioned in the synopsis given on page 405 is interesting as an example of the manner in which the sciences are mutually connected and dependent. Before the experiments of Fizeau in 1849, and of Foucault a few years later, our knowledge of the velocity of light depended on our knowledge of the dimensions of the earth's orbit: it had been found by astronomical observations upon the eclipses of Jupiter's satellites that light occupied a little more than 16 minutes in crossing the orbit of the earth, or about 8 minutes in coming from the sun; and hence, supposing the sun's distance to be 95,600,000 miles, as was long believed, the velocity of light must be about 192,000 miles per second; thus optics was indebted to astronomy for this fundamental element. But when Foucault in 1862 announced that, according to his unquestionably accurate experiments, the velocity of light could not be much more than 186,000 miles per second, the obligation was returned, and the suspicions as to the received value of the sun's parallax, which had been raised by the lunar researches of Hansen and Leverrier, were changed into certainty. The most recent experimental determinations of the velocity of light by Cornu in 1873–'74 fix the solar parallax between 8.80″ and 8.85″, according as we use Peters's "constant of aberration" or Delambre's value of the "equation of light," which is the name given to the time required for light to traverse the interval between the sun and the earth.

Collecting all the evidence at present attainable, it would seem that the solar parallax cannot differ much from 8.86″, though it may be as much as 0.04″ greater or smaller; this would correspond, as has

*by the square of the number of sidereal months in a year, and by the ratio between the masses of the sun and earth.* It is to be noted, however, that T and t are the periods of the earth and moon, as they would be if wholly undisturbed in their motions, and hence differ slightly from the periods actually observed—the differences are small, but somewhat troublesome to calculate with precision.

already been said, to a distance of 92,250,000 miles, with a probable error of about one-half per cent., or 450,000 miles.

But, though the distance can thus easily be stated in figures, it is not so easy to give any real idea of a space so enormous; it is quite beyond our power of conception. If one were to try to walk such a distance, supposing even that he could walk 4 miles an hour, and keep it up for 10 hours every day, it would take 68½ years to make a single million of miles, and more than 6,300 years to traverse the whole.

If some celestial railway could be imagined, the journey to the sun, even if our trains ran 60 miles an hour, day and night and without a stop, would require over 175 years. Sensation, even, would not travel so far in a human lifetime. To borrow the curious illustration of Prof. Mendenhall, if we could imagine an infant with an arm long enough to enable him to touch the sun and burn himself, he would die of old age before the pain could reach him, since, according to the experiments of Helmholtz and others, a nervous shock is communicated only at the rate of about 100 feet per second, or 1,637 miles a day, and would need more than 150 years to make the journey. Sound would do it in about 14 years if it could be transmitted through celestial space, and a cannon-ball in about 9, if it were to move uniformly with the same speed as when it left the muzzle of the gun. If the earth could be suddenly stopped in her orbit, and allowed to fall unobstructed toward the sun under the accelerating influence of his attraction, she would reach the central fire in about four months. I have said if she could be stopped, but such is the compass of her orbit that, to make its circuit in a year, she has to move nearly 19 miles a second, or more than fifty times faster than the swiftest rifle-ball; and in moving 20 miles her path deviates from perfect straightness by less than one-eighth of an inch. And yet, over all the circumference of this tremendous orbit, the sun exercises his dominion, and every pulsation of his surface receives its response from the subject earth.

By observing the slight changes in the sun's apparent diameter, we find that its distance varies somewhat at different times of the year, about 3,000,000 miles in all; and minute investigation shows that the earth's orbit is almost an exact ellipse, whose nearest point to the sun, or *perihelion*, is passed by the earth about the 1st of January, at which time she is 90,750,000 miles distant.

The distance of the sun being once known, its dimensions are easily ascertained—at least, within certain narrow limits of accuracy. The angular semi-diameter of the sun when at the mean distance is almost exactly 962″, the uncertainty not exceeding $\frac{1}{2000}$ of the whole. The result of twelve years' observations at Greenwich (1836 to 1847) gives 961.82″, and other determinations oscillate around the value first mentioned, which is that adopted in the "American Nautical Al-

manac." Taking the distance as 92,250,000 miles, this makes the sun's diameter 860,500; and the probable error of this quantity, depending as it does *both* on the error of the measured diameter and of the distance, is some 4,000 or 5,000 miles; in other words, the chances are strong that the actual diameter is between 855,000 and 865,000 miles.

Measurements made by the same person, however, and with the same instrument, but at different times, sometimes differ enough to raise a suspicion that the diameter is slightly variable, which would be nothing surprising considering the nature of the solar surface. There is no sensible difference between the equatorial and polar diameters, the rotation of the sun on its axis not being sufficiently rapid to make the polar compression (which must, of course, necessarily result from the rotation) marked enough to be perceived by our present means of observation.

It is not easy to obtain any real conception of the vastness of this enormous sphere. Its diameter is 108.7 times that of the earth, and its circumference proportional, so that the traveler who could make the circuit of the world in 80 days would need nearly 24 years for his journey around the sun. Since the surfaces of spheres vary as the squares, and bulks as the cubes, of their diameters, it follows that the sun's surface is nearly 12,000 times, and its volume, or bulk, more than 1,280,000 times, greater than that of the earth. If the earth be represented by one of the little three-inch globes common in school apparatus, the sun on the same scale will be more than 27 feet in diameter, and its distance nearly 3,000 feet. Imagine the sun to be hollowed out and the earth placed in the centre of the shell thus formed, it would be like a sky to us, and the moon would have scope for all her motions far within the inclosing surface; indeed, since she is only 240,000 miles away, while the sun's radius is more than 430,000, there would be room for a second satellite 190,000 miles beyond her.

The *mass* of the sun, or quantity of matter contained in it, can also be computed when we know its distance, and comes out 325,600 times as great as the earth. The calculation may be made either by means of the proportion given in the note to page 413, or by comparing the attracting force of the sun upon the earth, as indicated by the curvature of her orbit (about 0.119 inch per second), with the distance a body at the surface of the earth falls in the same time under the action of gravity, a quantity which has been determined with great accuracy by experiments with the pendulum. Of course, the fact that the sun produces its effect upon the earth at a distance of 92,250,000 miles, while a falling body at the level of the sea is only about 4,000 miles from the centre of the attraction which produces its motion, must also enter into the reckoning.[1]

[1] The calculation of the sun's mass, from the data given, proceeds as follows: Let M = the sun's mass, and m that of the earth; R = the distance from the earth to the sun,

This mass, if we express it in pounds or tons, is too enormous to be conceived: it is 2 octillions of tons—that is, 2 with 27 ciphers annexed; it is nearly 750 times as great as the combined masses of all the planets and satellites of the solar system—and Jupiter alone is more than 300 times as massive as the earth. The sun's attractive power is such that it dominates all surrounding space, even to the fixed stars, so that a body at the distance of our nearest stellar neighbor, *a* Centauri, which is more than 200,000 times remoter than the sun, could free itself from the solar attraction only by darting away with a velocity of more than 300 feet per second, or over 200 miles an hour; unless animated by a greater velocity than this, it would move around the sun in a closed orbit—an ellipse of some shape, or a circle, with a period of revolution which, in the smallest possible orbit, would be about 31,600,000 years, and if the orbit were circular, would be nearly 90,000,000. We say it would revolve thus—that is, of course, unless intercepted or diverted from its course by the influence of some other sun, as it probably would be. And we may notice here that in many cases certainly, and in most cases probably, the stars are flying through space at a far swifter rate, with velocities of many miles per second.

If we calculate the force of gravity at the sun's surface, which is easily done by dividing its mass, 325,600, by the square of $108\frac{3}{4}$ (the number of times the sun's diameter exceeds the earth's), we find it to be $27\frac{1}{2}$ times as great as on the earth; a man who on the earth would weigh 150 pounds, would there weigh nearly two tons; and, even if the footing were good, would be unable to stir. A body which at the earth falls a little more than 16 feet in a second would there fall 443. A pendulum which here swings once a second would there oscillate more than five times as rapidly, like the balance-wheel of a watch—quivering rather than swinging.

Since the sun's volume is 1,280,000 times that of the earth, while its mass is only 325,600 times as great, it follows at once that the sun's *average density* (found by dividing the mass by the volume) *is only about one-quarter that of the earth.* This is a fact of the utmost importance in its bearing upon the constitution of this body. As we shall see hereafter, we know that certain heavy metals, with which we are familiar on the earth, enter largely into the composition of the

and r the mean radius of the earth; T, the length of the sidereal year, reduced to seconds; and $\frac{1}{2}$ g the distance a body falls in a second at the earth's surface. Now, the distance the earth falls toward the sun in a second, or the curvature of her orbit in a second, is equal to $\frac{2\pi^2 R}{T^2}$ (about 0.119 inch). Hence, by the law of gravitation, $\frac{1}{2}g : \frac{2\pi^2 R}{T^2} = \frac{m}{r^2} : \frac{M}{R^2}$, whence, $M = m\left(\frac{4\pi^2 R^3}{T^2 r^2 g}\right)$.

In this formula make $\pi = 3.14159$; R, 92,250,000 miles; T = 31,558,149.3 seconds; r = 3,956.179 miles; and g = 0.0061035 mile (16.113 feet), and we shall get the result given in the text, viz., M = 325,600 m.

sun, so that, if the principal portion of the solar mass were either solid or liquid, its mean density ought to be at least as great as the earth's, especially since the enormous force of solar gravity would tend most powerfully to compress the materials. The low density can only be accounted for on the supposition, which seems fairly to accord also with all other facts, that the sun is mainly a ball of gas, or vapor, powerfully condensed, of course, in the central portion by the superincumbent weight, but prevented from liquefaction by an exceedly high temperature. And, on the other hand, it could be safely predicted on physical principles that so huge a ball of fiery vapor, exposed to the cold of space, would present precisely such phenomena as we find by observation of the solar surface and surroundings.

## EDUCATION AS A SCIENCE.

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I.

THE scientific treatment of any art consists partly in applying the principles furnished by the several sciences involved—as chemical laws to agriculture—and partly in enforcing, throughout the discussion, the utmost precision and rigor in the statement, deduction and proof of the various maxims or rules that make up the art.

Both fecundity in the thoughts and clearness in the directions should attest the worth of the scientific method.

Definitions of the Scope of Education.—First, let me quote the definition embodied in the ideal of the founders of the Prussian National System. It is given shortly as "the harmonious and equable evolution of the human powers;" at more length, in the words of Stein, "by a method based on the nature of the mind, every power of the soul to be unfolded, every crude principle of life stirred up and nourished, all one-sided culture avoided, and the impulses on which the strength and worth of men rest carefully attended to."—(Donaldson's "Lectures on Education," p. 38.) This definition, which is pointed against narrowness generally, may have had special reference to the many omissions in the schooling of the foregone times: the leaving out of such things as bodily or muscular training; training in the senses or observation; training in art or refinement. It further insinuates that hitherto the professed teacher may not have done much even for the intellect, for the higher moral training, nor for the training with a view to happiness or enjoyment.

Acting on this ideal. not only would the educator put more press-

ure altogether on the susceptibilities of his pupils: he would also avoid overdoing any one branch; he would consider *proportion* in the things to be taught. To be all language, all observation, all abstract science, all fine art, all bodily expertness, all lofty sentiment, all theology—would not be accepted as a proper outcome of any trainer's work.

The Prussian definition, good so far, does not readily accommodate itself to such circumstances as these—namely, the superior aptitude of individuals for some things rather than for others; the advantage to society of preëminent fitness for special functions, although gained by a one-sided development; the difficulty of reconciling the "whole man" with himself; the limited means of the educator, which imposes the necessity of selection according to relative importance.

Although by no means easy, it is yet possible to make allowance for these various considerations, under the theory of harmonious development; but, after the operation is accomplished, the doubt will arise whether much is gained by using that theory as the defining fact of education.

In the very remarkable article on education contributed by James Mill to the "Encyclopædia Britannica," the end of education is stated to be "to render the individual, as much as possible, an instrument of happiness, first to himself, and next to other beings." This, however, should be given as an amended answer to the first question of the Westminster Catechism—"What is the chief end of man?" The utmost that we could expect of the educator, who is not everybody, is to contribute his part to the promotion of human happiness in the order stated. No doubt the definition goes more completely to the root of the matter than the German formula. It does not trouble itself with the harmony, the many-sidedness, the wholeness, of the individual development; it would admit these just as might be requisite for securing the final end.

James Mill is not singular in his over-grasping view of the subject. The most usual subdivision of education is into physical, intellectual, moral, religious, technical. Now, when we inquire into the meaning of physical education, we find it to mean the rearing of a healthy human being, by all the arts and devices of nursing, feeding, clothing, and general regimen. Mill includes this subject in his article, and Mr. Herbert Spencer devotes a very interesting chapter to it in his work on Education. It seems to me, however, that this department may be kept quite separate, important though it be. It does not at all depend upon the principles and considerations that the educator, properly so called, has in view in the carrying out of his work. The discussion of the subject does not in any way help us in educational matters, as most commonly understood; nor does it derive any illumination from being placed side by side with the arts of the recognized teacher. The fact of bodily health or vigor is a leading postu-

late in bodily or mental training, but the trainer does not take upon himself to lay down the rules of hygiene.

The inadvertence, for so I regard it, of coupling the art of health with education is easily disposed of, and does not land us in any arduous controversies. Very different is another aspect of these definitions: that wherein the end of education is propounded as the promotion of human happiness, human virtue, human perfection. Probably the qualification will at once be conceded, that education is but one of the means, a single contributing agency, to the all-including end. Nevertheless, the openings for difference of opinion as to what constitutes happiness, virtue, or perfection, are very wide. Moreover, the discussion has its proper place in ethics and in theology, and, if brought into the field of education, should be received under protest.

Before entering upon the consideration of this difficulty, the greatest of all, I will advert to some of the other views of education that seem to err on the side of taking in too much. Here, I may quote from the younger Mill, who, like his father, and unlike the generality of theorists, starts *more scientifico* with a definition. Education, according to him, "includes whatever we do for ourselves, and whatever is done for us by others, for the express purpose of bringing us nearer to the perfection of our nature; in its largest acceptation, it comprehends even the indirect effects produced on character and on the human faculties by things of which the direct purposes are different; by laws, by forms of government, by the industrial arts, by modes of social life; nay, even by physical facts not dependent on the human will; by climate, soil, and local position." He admits, however, that this is a very wide view of the subject, and for his own immediate purpose advances a narrower view, namely: "the culture which each generation purposely gives to those who are to be its successors, in order to qualify them for at least keeping up, and, if possible, for raising, the improvement which has been attained."—("Inaugural Address at St. Andrews," p. 4.)

Besides involving the dispute as to what constitutes "perfection," the first and larger statement is, I think, too wide for the most comprehensive philosophy of education. The influences exerted on the human character by climate and geographical position, by arts, laws, government, and modes of social life, constitute a very interesting department of sociology, and have their place there and nowhere else. What we do for ourselves, and what others do for us, to bring us nearer to the perfection of our nature, may be education in the precise sense of the word, and it may not. I do not see the propriety of including under the subject the direct operation of rewards and punishments. No doubt we do something to educate ourselves, and society does something to educate us, in a sufficiently proper acceptation of the word; but the ordinary influence of society, in the dispensing of punishment and reward, is not the essential fact of education, as I

propose to regard it, although an adjunct to some of its legitimate functions.

Mill's narrower expression of the scope of the subject is not exactly erroneous; the moulding of each generation by the one preceding is not improperly described as an education. It is, however, grandiose rather than scientific. Nothing is to be got out of it. It does not give the lead to the subsequent exposition.

I find in the article "Education," in "Chambers's Encyclopædia," a definition to the following effect: "In the widest sense of the word a man is *educated*, either for good or for evil, by everything that he experiences from the cradle to the grave [say, rather, 'formed,' 'made,' 'influenced']. But, in the more limited and usual sense, the term education is confined to the efforts made, of set purpose, to train men in a particular way—the efforts of the grown-up part of the community to inform the intellect and mould the character of the young [rather too much stress on the fact of influence from without]; and more especially to the labors of professional educators or schoolmasters." The concluding clause is the nearest to the point—the arts and methods employed by the schoolmaster; for, although he is not alone in the work that he is expressly devoted to, yet he it is that typifies the process in its greatest singleness and purity. If by any investi gations, inventions, or discussions, we can improve his art to the ideal pitch, we shall have done nearly all that can be required of a science and art of education.

I return to the greater difficulty—namely, the question, what is the end of all teaching; or, if the end be human happiness and perfection, what definite guidance does this furnish to the educator? I have already remarked that the inquiry is acknowledged to belong to other departments; and, if in these departments clear and unanimous answers have not been arrived at, the educationist is not bound to make good the deficiency.

For this emergency, there is one thing obvious, another less obvious; the two together exhausting the resources of the educator.

The obvious thing is to fix upon whatever matters people are agreed upon. Of such the number is considerable, and the instances important. They make the universal topics of the schools.

The less obvious thing is, with reference to matters not agreed upon, that the educator should set forth at what cost these doubtful acquisitions would have to be made; for the cost must be at least one element in the decision respecting them. Whoever knows most about education is best able to say how far its appliances can cope with such aims as softening the manners, securing self-renunciation, bring ing about the balanced action of all the powers, training the whole man, etc.

We shall see that one part of the science of education consists in giving the ultimate analysis of all complex growths. It is on such

an analysis that the cost can be calculated; and, by means of this, we can best observe whether contradictory demands are made upon the educator.

What we have been drifting to, in our search for an aim, is the work of the school. This may want a little more paring and rounding to give it scientific form, but it is the thing most calculated to fix and steady our vision at the outset.

Now, in the success of the schoolmaster's work, the first and central fact is the plastic property of the mind itself. On this depends the acquisition not simply of knowledge but of everything that can be called an acquisition. The most patent display of the power consists in memory for knowledge imparted. In this view the leading inquiry in the art of education is how to strengthen memory. We are, therefore, led to take account of the several mental aptitudes that either directly or indirectly enter into the retentive function. In other words, we must draw upon the science of the human mind for whatever that science contains respecting the conditions of memory.

Although memory, acquisition, retentiveness, depends mainly upon one unique property of the intellect, which accordingly demands to be scrutinized with the utmost care, there are various other properties, intellectual and emotional, that aid in the general result, and to each of these regard must be had, in a science of education.

We have thus obtained the clew to one prime division of the subject—the purely psychological part. Of no less consequence is another department, at present without a name—an inquiry into the proper or natural order of the different subjects, grounded on their relative simplicity or complexity, and their mutual dependence. It is necessary to success in education that a subject should not be presented to the pupil until all the preparatory subjects have been mastered. This is obvious enough in certain cases: arithmetic is taken before algebra, geometry before trigonometry, inorganic chemistry before organic; but in many cases the proper order is obscured by circumstances, and is an affair of very delicate consideration. I may call this the analytic, or logical, department of the theory of education.

It is a part of scientific method to take strict account of leading terms, by a thorough and exhaustive inquiry into the meanings of all such. The settlement of many questions relating to education is embarrassed by the vagueness of the single term "discipline."

Further, it ought to be pointed out, as specially applicable to our present subject, that the best attainable knowledge on anything is due to a combination of general principles obtained from the sciences, with well-conducted observations and experiments made in actual practice. On every great question there should be a convergence of both lights. The technical expression for this is the union of the deductive and inductive methods. The deductions are to be obtained

apart, in their own way, and with all attainable precision. The inductions are the maxims of practice, purified, in the first instance, by wide comparison and by the requisite precautions.

I thus propose to remove from the science of education matters belonging to much wider departments of human conduct, and to concentrate the view upon what exclusively pertains to education—the means of building up the acquired powers of human beings. The communication of knowledge is the ready type of the process, but the training operation enters into parts of the mind not intellectual—the activities and the emotions; the same forces, however, being at work.

Education does not embrace the employment of all our intellectual functions. There is a different art for directing the faculties in productive labor, as in the professions, in the original investigations of the man of science, or the creations of the artist. The principles of the human mind are applicable to both departments, but, although the two come into occasional contact, they are so far distinct that there is an advantage in viewing them separately. In the practical treatise of Locke, entitled "The Conduct of the Understanding," acquisition, production, and invention, are handled promiscuously.

Bearings of Physiology.—The science of physiology, coupled with the accumulated empirical observations of past ages, is the reference in finding out how to rear living beings to the full maturity of their physical powers. This, as we have said, is quite distinct from the process of education.

The art of education assumes a certain average physical health, and does not inquire into the means of keeping up or increasing that average. Its point of contact with physiology and hygiene is narrowed to the plastic or acquisitive function of the brain—the property of fixing or connecting the nervous connections that underlie memory, habit and acquired power.

But as physiology now stands, we soon come to the end of its applications to the husbanding of the plastic faculty. The inquiry must proceed upon our direct experience in the work of education, with an occasional check or caution from the established physiological laws. Still, it would be a forgetting of mercies to undervalue the results accruing to education from the physiological doctrine of the physical basis of memory.

On this subject, physiology teaches the general fact that memory reposes upon a nervous property or power, sustained, like every other physical power, by nutrition, and having its alternations of exercise and rest. It also informs us that, like every other function, the plasticity may be stunted by inaction, and impaired by over-exertion.

As far as pure physiology is concerned, I invite everybody to reflect on one circumstance in particular. The human body is a great aggregate of organs or interests — muscles, digestion, respiration, senses, brain. When fatigue overtakes it the organs generally suffer;

when renovation has set in, the organs generally are invigorated. This is the first and most obvious consequence. It has next to be qualified by the remark that human beings are unequally constituted as regards the various functions; some being strong in muscle, others in stomach, others in brain. In all such persons the general invigoration is unequally shown; the favored organs receive a share proportioned to their respective capitals: to him that hath shall be given. Still more pertinent is the further qualification, that the organ that happens to be most active at the time receives more than its share; to exercise the several organs unequally is to nourish them unequally.

To come to the point as regards our immediate object. To increase the plastic property of the mind you must nourish the brain. You naturally expect that this result will ensue when the body generally is nourished: and so it will, if there be no exorbitant demands on the part of other organs, giving them such a preference as to leave very little for the organ of the mind. If the digestion or the muscles are unduly drawn upon, the brain will not respond to the drafts made upon it. Obversely, if the brain is so constituted by nature, or so excited by stimulation, as to absorb the lion's share of the nutriment, the opposite results will appear; the mental functions will be exalted, and the other interests more or less impoverished. This is the situation for an abundant display of mental force.

But we must further distinguish the mental functions themselves; for these are very different and mutually exclusive. Great refinement in the subdivisions is not necessary for the illustration. The broadest contrast is the emotional and the intellectual—feeling as pleasure, pain, or excitement, and feeling as knowledge. These two in extreme manifestation are hostile to each other: under extreme emotional excitement the intellect suffers; under great intellectual exertion the emotions subside (with limitations unnecessary for our purpose).

But intellect in the largest sense is not identical with the retentive or plastic operation. The laws of this peculiar phase of our intelligence are best obtained by studying it as a purely mental fact. Yet there is a physiological way of looking at it that is strongly confirmative of our psychological observations. On the physical or physiological side, memory or acquisition is a series of new nervous growths, the establishment of a number of beaten tracks in certain lines of the cerebral substance. Now, the presumption is that, as regards the claim for nourishment, this is the most costly of all the processes of the intelligence. To exercise a power once acquired should be a far easier thing, much less expensive, than to build up a new acquirement. We may be in sufficiently good condition for the one, while wholly out of condition for the other. Indeed, success in acquirement, looking at it from the physiological probabilities, should be the work of rare, choice, and happy moments; times when cerebral vigor is both abundant and well-directed.

BEARINGS OF PSYCHOLOGY.—The largest chapter in the science of education must be the following out of all the psychological laws that bear directly or indirectly upon the process of mental acquirement. Every branch of psychology will be found available; but more especially the psychology of the intellect. Of the three great functions of the intellect, in the ultimate analysis—discrimination, agreement, retentiveness—the last is the most completely identified with the education process; but the others enter in as constituents in a way peculiar to each. I will select for my present paper, DISCRIMINATION and RETENTIVENESS; and will endeavor to extract, from the discussion of these great intellectual functions, everything that they appear to yield for the ends of the educator. Although I can impart no novelty to the general statement of these functions, it is possible to make some unhackneyed remarks on their educational consequences.

DISCRIMINATION.—Mind starts from discrimination. The consciousness of difference is the beginning of every intellectual exercise. To encounter a new impression is to be aware of change: if the heat of a room increases ten degrees, we are awakened to the circumstance by a change of feeling; if we have no change of feeling, no altered consciousness, the outward fact is lost upon us; we take no notice of it, we are said not to know it.

Our intelligence is, therefore, absolutely limited by our power of discrimination. The other functions of intellect, the retentive power, for example, are not called into play, until we have first discriminated a number of things. If we did not originally feel the difference between light and dark, black and white, red and yellow, there would be no visible scenes for us to remember: with the amplest endowment of retentiveness, the outer world could not enter into our recollection; the blank of sensation is a blank of memory.

Yet further. The minuteness or delicacy of the feeling of difference is the measure of the variety and multitude of our primary impressions, and therefore of our stirred-up recollections. He that hears only twelve discriminated notes on the musical scale has his remembrances of sounds bounded by these; he that feels a hundred sensible differences has his ideas or recollections of sounds multiplied in the same proportion. The retentive power works up to the height of the discriminative power; it can do no more. Things are not remembered if they have not first been discriminated.

We have by nature a certain power of discrimination in each department of our sensibility. We can from the outset discriminate, more or less delicately, sights, sounds, touches, smells, tastes; and, in each sense, some persons much more than others. This is the deepest foundation of disparity of intellectual character, as well as of variety in likings and pursuits. If, from the beginning, one man can interpolate five shades of discrimination of color where another can feel but

one transition, the careers of the two men are foreshadowed and will be widely apart.

To observe this native inequality is important in predestining the child to this or that line of special training. For the actual work of teaching, it is of more consequence to note the ways and means of quickening and increasing the discriminating aptitude. Bearing in mind the fact that until a difference is felt between two things intelligence has not yet made the first step, the teacher is bound to consider the circumstances or conditions favorable and unfavorable to the exercise.

1. It is not peculiar to discrimination, but is common to every mental function, to lay down, as a first condition, mental vigor, freshness, and wakefulness. In a low state of the mental forces, in languor, or drowsiness, differences cannot be felt. That the mind should be alive, awake, in full force and exercise, is necessary for every kind of mental work. The teacher needs to quicken the mental alertness by artificial means, when there is a dormancy of mere indolence. He has to waken the pupil from the state significantly named *indifference*, the state where differing impressions fail to be recognized as distinct.

2. The mind may be fresh and alive, but its energies may be taking the wrong direction. There is a well-known antithesis or opposition between the emotional and the intellectual activities, leading to a certain incompatibility of the two. Under emotional excitement, the intellectual energies are enfeebled in amount, and enslaved to the reigning emotion. It is in the quieter states of mind that discrimination, in common with other intellectual powers, works to advantage. I will afterward discuss more minutely the very delicate matter of the management of the various emotions in the work of teaching.

3. It must not be forgotten that intellectual exercises are in themselves essentially insipid, unattractive, indifferent. As exertion, they impart a certain small degree of the delight that always attends the healthy action of an exuberant faculty; but this supposes their later developments, and is not a marked peculiarity in the child's commencing career. The first circumstance that gives an interest to discrimination is pleasurable or painful stimulus. Something must hang on a difference before the mind is made energetically awake to it. A thoroughly disinterested difference is not an object of attention to any one.

The transitions from cold to hot, dark to light, strain to relief, hunger to repletion, silence to sound, are all more or less interesting, and all more or less impressive. But then they are vehement and sensational. It is necessary, in order to the furnishing of the intelligence, that smaller and less sensational transitions should be felt; the intellectual nature is characterized by requiring the least amount of emotional flash in order to impress a difference. A loud and furious

demonstration will certainly compel attention and end in the feeling of difference, but the cost is too great to be often repeated.

4. The great practical aid to the discovery and the retention of difference is immediate succession or, what comes to the same thing, close juxtaposition. A rapid transition makes evident a difference that would not be felt after an interval, still less if anything else were allowed to occupy the mind in the mean time. This fact is sufficiently obvious, and is turned to account in easy cases, but is far from thoroughly worked out by the teacher and the expositor. Any trifling diversion will suffice to blind us to its importance.

We compare two notes by sounding them in close succession; two shades of color by placing them side by side; two weights by holding them in the two hands, and attending to the two feelings by turns. These are the plain instances. The comparison of forms leads to complications, and we cease to attempt the same kind of comparison. For mere length we lay the two things alongside; so for an angle. For number, we can place two groups in contiguous rows—three by the side of four or five—and observe the surplus.

Mere size is an affair of simple juxtaposition. Form, irrespective of size, is less approachable. A triangle and a quadrangle are compared by counting the sides, and resolving the difference of form into the simpler element of difference of number. A right-angled, an acute-angled, an isosceles triangle, must be compared by the juxta position of angles. A circle and an oval are represented by the alternatives of curvature and diameters: in the one, the curvature uniform and the diameters equal; in the other, the curvature varying and the diameters unequal. The difference between a close and an open curve is palpable enough.

The geometrical forms are thus resolvable into very simple bases of comparison; and the teacher must analyze them in the manner now stated. For the irregular and capricious forms, the elementary conceptions are still the same—lineal size, number, angular size, curvature—but the mode of guiding the attention may be various. Sometimes there is a strong and overpowering similarity, with a small and unconspicuous difference; as in our ciphers (compare 3 and 5), and in the letters of our alphabet (C, G), and still more in the Hebrew alphabet. For such comparisons, the difference, such as it is, needs to be very clearly drawn or even exaggerated. Another method is to have models of the same size to lay over one another, so as to bring out the difference through the juxtaposition. By an express effort, the teacher calls on the learner to view, with single-minded attention, the differing circumstance, and afterward to reproduce it by his own hand. An express lesson consists in asking the pupil what are the ciphers, or the letters, that are nearly alike, and what are the points of difference.

The higher arts of comparison to impress difference are best illus-

trated when both differences and agreements have to be noted. They would have to be resumed after the discussion of the intellectual force of agreement or similarity. The chief stress of the present explanation lies in regarding discrimination as the necessary prelude of every intellectual impression, as the basis of our stored-up knowledge, or memory. Agreement is presupposed likewise; but there is not the same necessity, nor is it expedient, to follow out the workings of agreement, before considering the plastic power of the intellect.

## THE PRODUCTION OF COGNAC BRANDY.

THERE is a small district in the south of France known as the Deux Charentes, which has a commercial centre called Cognac. From the grapes of this district there comes a wine, and from this wine there is distilled a celebrated liquor which is named after the place, and called *Cognac brandy*. This spirit, *eau de vie supérieure*, as the French call it, is liked by a great many people, and hated by a great many more, so that it may fairly be assumed as an object of general interest. A writer in the *Pall Mall Gazette* has been at the pains to collect a large amount of information concerning it, to which we are indebted for the substance of the following statements.

England consumes by far the greater part of the supply; English firms practically control the export trade; and English influence is so potent in Cognac, that the rural population of the department speak jocularly of the place as the "little English town on the river Charente."

The Cognac-brandy district begins at Angoulême, about three hundred miles south of Paris, and comprises from fifty to sixty square miles. It is divided into five parts, and is cut in two from east to west by the river Charente. The parts are, in the order of their importance as established by the quality of the brandy they produce, though in the inverse order as to size, as follows: the Grande Champagne; the Petite Champagne; the Borderies, a strip of land along the banks of the Charente opposite the Grande Champagne; the Fins Bois; and the Bon Bois. The country is undulating. The surface, dotted with towns and villages, and diversified by occasional tracts of woodland between bright-green pastures on either bank of the river, is divided into fields spotted with walnut-trees and vineyards, with red-roofed farm-houses, and traversed by broad roads lined with rows of tall elms and poplars. The soil is principally clayey and flinty rock, supported by a bed of chalk or limestone, and occasionally of marl, that in the Grande and Petit Champagnes being of the best quality.

*Eau de vie* is a French term equivalent to the English word spirits, and hence is applicable to alcohols derived from any source. But the *eau de vie de Cognac* is the spirits obtained by distillation of the fermented juice of a few varieties of grape, chief among which is *la folle blanche* as it is called. This is a white grape. The name, which means literally "*the white fool*," is probably due to the fact that the *folle blanche* produces only a very inferior wine, which commands but eight cents a gallon, while a common red wine brings sixteen.

In the Deux Charentes there are three kinds of vineyards, called "*vignes pleines*," "*vignes en allées*," and "*vignes à bœufs*." In each the vines are planted in rows, which in the first are five feet apart. Hand-labor is generally employed in the cultivation, though the plough is used to loosen the ground where the rows are wide enough apart. The *vignes en allées* consist of long, narrow strips of land planted with vines in rows, every fourth or fifth row or so having a slip of ground sown with grain or vegetables in between. In these vineyards, which are more common in the Grande Champagne, the vines as a rule are planted rather wide apart. The *vignes à bœufs* are so termed from the rows being wide enough apart (from five to six feet) to admit of oxen and a plough passing between. The vines, as a rule, are left without supports. The producers are mostly small farmers, who cultivate their own vineyards, with little if any help. When help is employed, the wages vary from two to three francs a day, according to whether meals are furnished or not. These peasant proprietors are a frugal, saving class, and are not uncommonly rich.

It is unpleasant to relate that a speedy and almost complete suspension of this important industry is threatened in the ravages of the *Phylloxera vastatrix*, a minute and (to the naked eye) invisible insect, that preys on the roots and leaves of the vine, to the unfailing destruction of the plant. Large rewards offered by the French Government have had the effect of calling forth a number of remedies, but none of them have proved efficacious. During the year just past the insect spread nearly all over the Deux Charentes and reduced the vintage, so that it nowhere amounted to more than one-half a crop, and in some places not more than a tenth, the average being about one-sixth. Many farmers, in despair, actually cleared their fields and sowed them in grain. In many places a large part of the vines have been killed and the influence of the scourge was to cause a general neglect of the vineyards.

The grapes are picked, for the most part, by women wearing high-crowned fluted caps, who use a hook-shaped knife to sever the stems. Each carries with her a small wooden box with sloping sides, into which the fruit is thrown. When these boxes become full they are emptied into the baskets of the men who carry the grapes to the cart at the edge of the vineyard. The carts have long bodies and very high wheels. and a huge tub, fixed between four upright stakes. The

carriers, bending beneath their heavy loads, mount a ladder to the top of the tub, and by a peculiar twist of the body empty their baskets of grapes into it. Within the tub is a lad, who treads upon the grapes to reduce their bulk, and, in a measure, press out their juice. The cart being loaded, is drawn off by a yoke of oxen to the neighboring press-house. The grapes are next emptied, through an opening in the wall, upon a sloping stone floor, where they are crushed by an ordinary grape-mill, which, however, forces out only a portion of their juice. Formerly the juice was trodden out by the feet of the laborers. It runs down the sloping floor into a covered trough at the lower end, by which it is led into a tank—whence it is emptied into the casks, and then left to ferment.

As already said, the mill does not express all the juice from the grapes, and so the "must" is shoveled through an opening in the wall into a large, shallow trough at the foot of the press. Then it is heaped up in the centre of the trough, into what is called the *motte*, a form like a millstone, and subjected to powerful pressure. The sides of the *motte* are now trimmed, the screw loosened, and the trimmings piled on the top, when the pressure is again applied. This process is repeated until the must has been subjected to four pressures. Each pressure lasts about two hours, except the last, which, being generally put on in the evening, continues all night. Next day the must is spread out in the trough, watered from a watering-pot, and raked about in the water for an hour. The water being drawn off, the must is again put under pressure, and the juice obtained is mixed with the water, and the whole put into a cask to ferment.

The must, or juice, obtained from the milling and four previous pressures is put in casks, vats, or cisterns, to ferment, and it is from it that the *eau de vie supérieure* is obtained. The yield of fermented liquor in good seasons is, in the Grand Champagne, about 900 gallons to the acre; in the Deux Charentes, as a whole, about 500 gallons; and in some parts of the Bon Bois as low as 200 gallons. And, although, as already stated, the vineyards are generally small, crops of 20,000 to 50,000 gallons from particular ones were formerly known.

It is to be observed that the method of fermenting the wine intended for the distillation of brandy differs a little from that pursued with the red wine of the district: the murk being allowed to remain in the juice in the last case, while it is not allowed to do so in the first.

The still comprises a reservoir, with a pump for supplying it from a large stone tank below, and the usual furnace and retort, with head and worm. The average capacity of the stills throughout the Grand Champagne is only about fifty-five gallons at a single operation. The wine to be distilled having been emptied into a square stone tank, already referred to, is pumped into the reservoir, whence, through a tap, it is conveyed into the retort, which is heated with coal, at first

to a high degree, and afterward to a lower. At the end of several minutes a few drops of white, translucent liquid issue from the pipe of the worm, increasing soon to a little streamlet, which falls into a small cask. This liquid contains about half its weight of water, and is called the *brouillis*. It continues to flow until it becomes gradually less alcoholic, when a momentary pause occurs in the operation. A tap at the bottom of the retort is opened and the boiled wine, a brownish liquid, is either put back into the reservoir or allowed to run away. The wine from the reservoir is then turned into the retort until the latter is about two-thirds full. The same process is repeated, day and night, until all the wine has been converted into *brouillis*, which, being rectified, is then ready for delivery to the Cognac-brandy shippers as *eau de vie*. The proportion of brandy yielded by the wine is not fixed, but variable with certain circumstances. In a vintage of good quality it is one gallon of brandy to six or eight of wine; but in unfavorable seasons it is not more than one to seven and a half or twelve. Newly-made wine furnishes more spirit than wine twelve months old; and wine fermented in large bulk more, in proportion, than that fermented in small casks.

Cognac brandy is at first a colorless liquid, but it gradually acquires a pale yellow or amber color from the cask in which it is kept for ageing. With its natural appearance, however, it never appears to the consumer; public taste having become vitiated to the extent of requiring a rich brown or brandy color, which is imparted by a mixture of caramel or burnt sugar. Occasionally, too, a little red sanders-wood is used for coloring. The constituents are alcohol and water and small quantities of volatile oil, acetic acid, acetic ether, œnanthic ether, tannin, etc., and, as it reaches the consumer, coloring matter. The quantity of alcohol varies from 48 to 55 per cent.; the latter being the standard strength, or "proof." It is generally imported into England at 1 to 3 over proof, but the strength is lessened by age, so that, when taken from bond for sale, it seldom exceeds 3 or 4 under proof. The quality of the brandy depends not, as may be generally supposed, on the quantity of alcohol it contains, so much as on the minor constituents, notably the œnanthic ether, from which it derives its distinguishing smell and flavor. This fact becomes apparent when it is reflected that, while brandy, as is well known, improves with age, it loses thereby a part of its alcoholic strength. The very finest brandies, in fact, average from 5 to 10 under proof, and never rise above 2 under proof. In this connection, one or two interesting facts may be noted. It has already been stated that the grape from which the finest Cognac brandy is obtained yields at best an inferior wine. Now, the best wine-making grapes contain a comparatively large proportion of sugar, which varies from 12 to 26 and 30 per cent., and it is the sugar that in fermentation is converted into alcohol. The *folle*

*blanche*, however, contains a relatively small quantity of sugar, or only about 7 to 8 per cent. Again, the riper the grape the more sugar it will contain, but experience has taught the vine-dressers of the Deux Charentes that, if their grapes are allowed to thoroughly ripen, the brandy produced is stronger, but proportionally inferior in quality. So that all the facts lend confirmation to the statement just made.

It was remarked, a little while ago, that the quality, or "bouquet," of the brandy—that is, its peculiar odor—was derived from œnanthic ether. This ether is obtained from the seed of the grape, and, according to Neubauer, is a combination of various substances, of which caprylic and caproic acid ethers are the most important part.

The strength at which Cognac brandy is sold in England to consumers is from 11 to 12 under proof, to which it is lowered by the addition of water, after, it is said, it has passed into the hands of wholesale and retail dealers. The standard recognized in the brandy-trade is 10 under proof, and it is never lowered beyond 12 under proof, except by special agreement. Below 17 under proof it is seizable by the English excise.

It is the opinion of those, who have investigated the matter, that very little, if any, adulteration is practised before the brandy is shipped from France. Heavy penalties, imposed by the tribunals on certain Charente farmers, who some years ago were detected in the practice of doubling the quantity of their brandy by sophistication, have operated to prevent other farmers from falling into like practices; and a still more powerful deterrent is that no farmer can adulterate his brandy without making it known to his neighbors. In France no wine or spirit can be moved about without an official permit, and a distiller in the Charente could not receive a cask without everybody knowing it; so that any one who procured a raw spirit would at once become a marked man, and excluded from doing business with shippers. It is said that the farmers now confine their attempts to cheat to overstating the age of the brandy they offer for sale.

Besides water, the adulteration is chiefly made with inferior spirit. In addition to the dishonesty, there is much injury to health and life involved in the practice. There is a kind of alcohol known as *amylic*, or fusel-oil, contained in the spirits obtained from every substance except the grape, but in particularly large quantities in the spirits of potatoes, beet-root, and Jerusalem artichoke, which, being inferior, are those chiefly used for adulteration. This fusel-oil is a deadly poison. Says Dr. G. O. Drewry, "The public would not drink such a poison at any price if they were once awakened to a sense of its terrible nature." It is never formed in the presence of tartaric acid, which, as is well known, abounds in grape-juice; hence, spirits distilled from the pure juice of the grape contain no fusel-oil whatever.

It remains, before closing this paper, to speak of the manner of

collecting the brandy into the shipping-houses in Cognac, and of the treatment it receives therein. It is bought up from the distillers by commission-merchants, or shippers, who have large storehouses, supplied with facilities for filtering, mixing, ageing, etc. The two largest houses in the trade are those of the celebrated English firms, Martel & Co. and Hennessy & Co.; next to these are Otard, Dupuy & Co., and Augier Frères, the oldest house in Cognac; and besides these are many smaller ones. The farmers generally sell their spirit while it is new, or immediately after distillation. In the second year it is classed "stale," and in the fourth "old." On its arrival at the magazine it is tested by a sampler, to see that it corresponds with the representations made for it. It is measured in large *dépotoirs*, about 265 gallons capacity, which have glass tubes on the outside to indicate the quantity of spirit within. This is said to give a fairer measurement than smaller vessels. The price depends on the strength. For the English market this is classed at 58° of Gay Lussac's scale, or about 1° above English proof. For the French market it is 60°, and for the American 61°. It may as well be stated in this connection that England takes nearly the whole supply—her portion in 1875 amounting to 4,500,000 gallons. A small part is consumed in France, and other small parts go direct to the north of Europe, South America, and the United States.

Sales of spirits are based on the French scale—or 60°. For each degree above that standard the shipper pays the producer 5 per cent. extra; while for each degree under he deducts 10 per cent. After measurement the spirit is placed in new oaken casks, of the proper seasoning, and its age, quality, and origin, are indicated thereon. The casks are stored in a series of chambers threaded with tramways for moving them about; and in a vast gallery, beneath, stand long rows of conical-shaped colossal white vats, each more than twelve feet high and nine feet in diameter at its base. These are for use in mixing. By mixing, the peculiarities of the different varieties of spirit are blended, and so the "brands" are multiplied. In this process the various kinds are emptied, first, into a copper-plated trough on the floor above the vats, and at the same time passed through a filter of flannel; then it is drawn off into the vat below, passing in its course through a second filter of white blotting-paper, surrounded by flannel. In the vats it is stirred by paddles—in some houses worked by hand and in others by machinery. This completes the mixing, and the spirit is drawn off into casks or bottles and stored for shipment. A part of the alcohol, estimated at 7 to 10 per cent., is lost by evaporation in the first year of ageing, and a considerably smaller part in subsequent years. The visible effect of this evaporation is displayed in the carbonized appearance of the walls and roofs of the older stone houses—a sootiness which the stranger is sure to attribute to the smoke of the distilleries, of which, however, there are none in Cognac.

# UPS AND DOWNS OF THE LONG ISLAND COAST.

By E. LEWIS, Jr.

"Daily it is forced home on the mind of the geologist that nothing, not even the wind that blows, is so unstable as the crust of this earth."—Darwin, in 1835.

OBSERVATIONS made around the shores of Long Island justify the conclusion that they have undergone important changes in time geologically recent. These changes appear to have arisen from a series of vertical movements, by which the coast has been alternately elevated and depressed.

In consequence of these movements the shore-line of the island has advanced and again receded, perhaps, in repeated instances: being at one time, upon the ocean-side, from fifty to seventy miles southward of where the waves now break; while at another period the highest hills of the island were largely if not wholly submerged. The persistence and extent of these movements are interesting and important questions in geology. We do not know at present how great the oscillations of the coast may have been, but enough is obvious, in the records they have left in the contour and structure of the island, to show that they have been much greater than is indicated on the adjacent mainland of Southern New England. We shall endeavor to follow these records, obscure and perplexing though they sometimes are, back to the period in which Long Island may be said to have had its origin—a period which witnessed the approach and presence of a great ice-sheet upon this coast.

It is not questioned, we believe, that Long Island is a terminal glacial moraine, and that the material of which it is composed is the *débris* of regions over which the ice moved in its progress toward the sea. Its underlying portions are beds of laminated sands and clays which have been referred to periods antecedent to the advent of the ice, and which constitute in one sense a part of the island. Its great mass, however, overlies these beds, and presents two general forms of structure. One is known as the "unmodified bowlder-drift," in which there are no stratified beds; the other is the "modified drift," or that in which the material has been distributed in layers chiefly by the action of waves. Much of the hill-region of the island presents the peculiar pell-mell structure of the one—the stratified gravels and sands of Southern Long Island are typical of the other. These differences in structure, and other facts to be mentioned, imply great changes in the relative level of land and sea upon the coast.

In considering these movements of oscillation it will be convenient to notice the latest first, and others in their order. A persistent invasion of the ocean upon the shores of the island has taken place

in recent time, in consequence of which its bluffs have been undermined by waves, and the lowlands submerged. Immense bowlders lie upon some portions of the shores, which indicate the sites of recent banks and headlands.

The wearing away of an exposed coast, like that of the east end of Long Island, suggests a subsidence, by which it is continually being brought under dominion of the waves. Did no such change occur, the abrasion would be retarded, or might finally cease, unless, indeed, the falling material be removed by a coastwise drift of the water. The tendency of wave-motion is to throw upon shore the waste of the cliffs, thus raising a breakwater on which the waves expend their force. But, where low grounds along the ocean-margin become permanently overflowed, the proof is conclusive that a change of relative level has taken place. There has been no abrasion by waves, but silently and imperceptibly the tides have advanced upon the uplands.

Around the shores of Long Island are large areas of recent forest, swamp, and meadow, with remains of their peculiar forms of vegetation in many cases undecayed, covered by water to depths of from one to sixteen or more feet. Some facts illustrating this were presented to the Natural History Section of the Long Island Historical Society, in May, 1868, a synopsis of which was published in the *American Naturalist* for August of that year. A few of these, with others of importance since discovered, are offered in this paper.

The movement under consideration is by no means a local one, but occurs along the Atlantic border from Labrador to the Capes of Delaware, and in a lesser degree to Florida. Prof. G. H. Cook, in his admirable "Report on the Geology of New Jersey," cites many instances along the coast of that State where swamps of cedar and other forms of vegetation are now submerged, or covered with salt meadow.

On the south side of Long Island are about 40,000 acres of salt marsh and meadow. Their vast stretches of level surface, fringed by the Great South Bay and the beach on the one side, and by uplands on the other, present a scene of rare and surpassing beauty. The meadow rests upon a floor of gravel and sand. It varies in thickness from a few inches at the uplands to eight or ten feet near the beach, and is filled with the roots of grasses throughout. It has been formed by growth and accumulation at the surface, for the meadow-grasses thrive only at or near high-tide level where rainfall and sunshine can reach them. The increase of the meadow in thickness has, therefore, just kept pace with the deepening of the water, or in other words, with the sinking of the coast.

Beneath the meadows remains of swamp and forest are found. These are fast rooted, and often six feet beneath the surface. At Islip a great number of stumps are found in the salt meadow of Wil-

liam Nicol, Esq., remains of a forest, portions of which are still flourishing on the adjacent uplands. These, Mr. Nicol writes, "are of oak, and are from twelve to twenty-four inches in diameter." Similar ones, he is informed, occur on the north side of the beach close to the ocean, which are covered by three feet of water at low tide. Eastward from this point the bay is broad and shallow for upward of twenty miles. The depth of water in it is from three to eight feet, with from two to three feet more in some parts of the channel. A tradition of the early settlers, which appears to have been received from the aborigines, is, that the whole area was once a fresh-water swamp, portions of which were so nearly dry at certain seasons of the year, that the Indians passed over it dry-footed to the beach.

A hundred and fifty years ago the bottom of this bay was covered in many places with remains of swamp vegetation, and stumps of trees, to the "great annoyance and astonishment of fishermen."

It is probable that this section of the bay was at one time a swamp or series of swamps like many now found on the contiguous uplands, and sufficiently above the level of the sea to admit of their free drainage into it, for it is certain that they were supplied by the same copious streams from the island which now empty into the bay.

The character of these swamps changed when the tides overflowed them. That the bay is comparatively modern is suggested by the fact that no great mounds of shells[1] occur near it, such as were left by the aborigines along other parts of the coast. Yet it is certain that the country was thickly settled by Indians. Mr. Nicol writes, "There are fields known as old Indian fields which abound in shells, but they nowhere take the form of mounds."

A few miles eastward is the beautiful but shallow sheet of water known as Tiana Bay. It fills a depression in the almost level sands along this part of the coast, and is upon the site of a pine-forest. W. S. Pelletreau, Esq., of Southampton, informed us that he saw in it about three hundred stumps covered at low tide. They are of the same species of pine which now grows on the adjacent uplands.[2]

In Peconic Bay, which divides the eastern part of Long Island into two very long necks of land, the submergence of the shores has been extensive. Mr. E. F. Squires, of Riverhead, noticed not only areas of swamp, but of former cedar-forests, now permanently overflowed by the tides. One point is known as "Stump Landing."

On the north shore of the island are several tracts of "sunken meadow," over which the water at low tide is from ten to fifteen feet

[1] Westward of this portion of the shore of the "Great South Bay" are many "Indian shell-heaps," all of them now surrounded by meadows. Some of them, six or more feet deep, near the margin of the ocean, are covered by every tide. These are probably very old, and were formed originally at the uplands.

[2] The pitch-pine (*Pinus rigida*). This tree almost rivals the maritime pine of Europe in flourishing at the verge of salt-water. Emerson states that it is not killed by occasional overflow of the tides.

deep. These dead and submerged meadows are but little decayed, and are usually continuous with those now growing upon the shores.

On the flat shores of the south side of the island the encroachment of meadow upon the uplands is attended with interesting results. It forms first in depressions where the tides overflow. In this way knolls of upland, cultivated or perhaps covered with trees, become islands, which are in turn overflowed and covered by meadow, but some of the more elevated ones remain almost at the verge of the ocean. Barnum's, formerly Hog Island, in East Rockaway Bay, now being converted into an asylum for the paupers of Queen's County, is one of these.

It is well known that the beach [1] on which the ocean breaks is gradually thrown inland upon the meadows. By this means old meadows are sometimes laid bare. It is stated, in Furman's "Antiquities of Long Island," that when Jones's Inlet was opened through the beach during a storm, it was found that the "bottom, laid bare, was solid meadow, in which were tracks of cattle, or of cloven-footed beasts."

The old meadow-bottoms, and sometimes masses of the tangled roots of upland vegetation, are torn up by waves during storms and thrown upon the beach. We have seen this turfy matter lying like windrows along the surf. Mr. Pelletreau informs us that just opposite the east end of Shinecock Bay "there was washed out by waves a large quantity of what is called meadow-bottom, partially decomposed vegetable matter, remains of fresh-water plants. . . . A few years since a violent storm washed away the sea-beach near Southampton, exposing at low tide, nearly at the brink of the ocean, a row of fence-posts that were put down by the first settlers." From these and other facts this careful observer concludes that the ocean, in that vicinity, has encroached upon the land about half a mile in two hundred years.

At Montauk Point, north of the lighthouse, is a low, swampy place, over which the tides sometimes rise. We are informed by Mr. J. F. Gould, who was for many years keeper of the lighthouse, that stumps are laid bare in front of this swamp, at the sea-margin, when the tide is extremely low. A similar phenomenon occurs at the extreme westerly end of the island. A few rods south of Fort Hamilton, at the entrance of New York Harbor, are the well-known Dyker Meadows (Fig. 1). They occupy the site of a swamp which is filled with the remains of upland and fresh-water vegetation.

The swamp was originally about a mile long, and was in one of the valley-shaped depressions common on the surface of the Long Island drift. It lost its character as a swamp by encroachment of the tides upon it, and was finally converted into a salt-marsh. This

[1] This breakwater of sand extends from Coney Island to the hills of Montauk, a distance of nearly one hundred miles.

marsh abounds in stumps; a great number of large size, some of them three feet in diameter, have been seen by the writer at the verge of low water, and we have found them many rods from the shore where the water was ten feet deep.[1] This area was certainly a portion of the original swamp when the land was sufficiently elevated to lift it above the level of the sea.

It is not necessary to further illustrate the present subsidence of this coast, but evidence of the extent of the movement will be of interest.

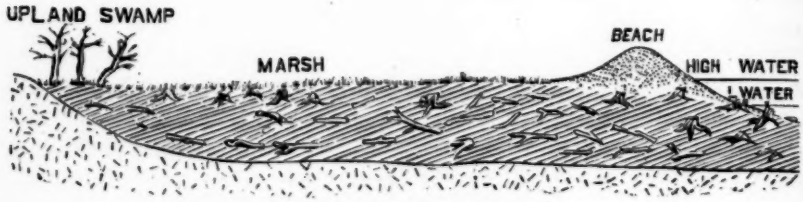


FIG. 1.—SECTION THROUGH THE DYKER MEADOWS.
Horizontal scale, four inches to the mile; vertical scale, twenty feet to the inch.

In constructing the Erie Basin at Brooklyn in New York Harbor, Mr. George B. Brainerd, engineer, found the following series of deposits, the water being ten feet deep at low tide: Three and a half feet of mud, sand, etc.; ten feet compact peaty meadow. This gives twenty-three and a half feet of depression since the bottom of that meadow was the surface, and covered with vegetation at the level of the sea.

In 1867 John Nadir, Esq., United States Engineer at Fort Hamilton, carefully examined, by boring, the underlying formation around Fort Lafayette. The earth was penetrated to a depth of 53 feet at points between 800 and 1,000 feet from the shore, where the water at low tide was ten feet deep. The deposits passed through were as follows: twenty feet coarse sand and gravel, with few broken shells; three feet decayed meadow, with shells and *Diatomaceæ*; seventeen feet gravel and sand, with broken shells; thirteen feet mud, quite compact, which appears to have been a marsh with scanty vegetation, and shells.[2] This indicates a subsidence of the coast of at least sixty-three feet, or, in other words, the land of the coast was that number of feet higher than it now is, when the subsidence began. But there is reason to conclude that the elevation was much greater than sixty-three feet. If we take a step backward in the order of events, we find that, immediately previous to the elevation mentioned, there occurred a great depression of the coast. Possibly the highest hills of the

[1] "Cedar-swamps, buried beneath the meadows on the New Jersey coast, have yielded logs six feet in diameter, and some with 1,000 rings of growth."—*Prof. Cook's Report.*

[2] The shells were identified by Mr. A. R. Young, of Brooklyn, as follows: *Nassa obsoleta, Anomia ephippium, Mya arenaria, Crepidula fornicata, Solen ensis,* and *Mytilus edulis.*

island were carried under water. The evidences of this depression are found in the numerous beds of stratified sand and gravel—elevated beaches and other shore-formations—which lie along the central ridge of hills,[1] and fronting the ocean from 100 to 260 or more feet above the level of the sea. At whatever heights these deposits occur, they suggest, if they do not prove, submergence of the coast to that extent.

From observations, made by the writer and others, it is ascertained that the summit of Hempstead Harbor Hill, which is 384 feet above tide, and the highest land upon Long Island, is a mass of stratified sand and gravel. The same is true of Janes Hill, in the West Hill group, said to be 383 feet high, and of Osborn's Hill, southwest of Riverhead, the height of which, according the United States Coast Survey, is 293 feet. In these instances, and in others similar, the layers are distinct and well defined. The stratification of this material was evidently the work of waves, but, whether of the ocean, or of a glacial lake or sea, admits of doubt. At present we cannot determine what the extent, contour, or elevation of the surface around these dome-like hills may have been; nor can we tell the original extent of the beds of assorted material, remains of which now cap the hills. That the denudation has been immense, is everywhere evident.

From the summits of the hills mentioned one overlooks southward a vast plain which extends to the ocean, ten miles distant; and the conclusion seems irresistible that every rood of that distance has been the shore-line of first an invading, afterward of a receding ocean, and the scene of those great coast-changes which waves produce. We may restore, in imagination, the hills of glacial rubbish crumbling before the stroke of waves, as during an immense period of time the subsidence of the land went on. So complete was the work of disintegration, that scarcely a bowlder remains in the low tracts fronting the ocean, but are numerous along the margin of the hills, and abound in the undisturbed drift[2] which constitutes much of the hill-region.

The period of subsidence we are considering is referred to the "Champlain" of the geologists, so called by Prof. C. H. Hitchcock from the abundance of its peculiar deposits near that lake. It is a marked one in geological history. During its progress the deposits

[1] A ridge of hills, varying in height from one hundred and fifty to three hundred and eighty-four feet above tide, extends, with some interruptions, through Central Long Island. They are drift with bowlders; but nowhere show rock in place, as some have supposed.

[2] Many bowlders on Long Island are of immense size; one, at Manhassett, contains upward of 20,000 cubic feet. Two others now or recently in the same valley are, in circuit, 108 and 126 feet respectively. One, on Strong's Neck, in Suffolk county, has a content of 14,000 cubic feet. Bowlders are found on the tops of the highest hills, and form an enormous rip-raps in some places where they have fallen as the banks were undermined by waves.

which the glaciers had left were altered in their contour, and redistributed by waters which encroached upon and finally covered them. The material thus redistributed would be left in layers, chiefly of gravels, sands, and clayey sands, if upon the ocean-margin, as is the case in Long Island.

In sinking wells into the beds formed during this period of subsidence, wood and shells have been found in a great number of instances. The wood sometimes occurs in logs of large size; oak and pine have been identified. We have a record of sixteen instances where wood has recently been found, and many others are mentioned by Thompson in his history, and by Mather in his excellent report on the geology of the island. These facts seem to imply that forests were upon the adjacent lands, which was not the case during the presence of glaciers upon the coast.

The shells found are at various depths—in one case at Gravesend 100 feet below the surface—and occur in all parts of the island where the stratified drift abounds. They have been found at Flatbush, Prospect Park, Bath, East New York, Farmingdale, Amagansett, and elsewhere.

The beds in which they occur are not of the low plains only, but many feet above tide. At Manhassett, as we are informed by John M. Clark, Esq., of that place, a well was dug, and at several feet below the surface-rubbish a layer of what appeared to be "creek-mud" was found, in which were a great number of shells of the oyster, clam, and scallop, many of them unbroken. The layer was about five feet thick, and throughout contained not only shells, but leaves, pine-cones, also wood of pine and other species. This interesting deposit is about 200 feet above tide; but the contour of the present surface indicates plainly enough that an arm of a bay (Little Neck Bay) contiguous extended over this area when the land was sufficiently submerged to admit of it.

It is obvious that only the portion of Long Island which is more than 200 feet above tide was at that time dry land.

But the subsidence was greater than is indicated by this elevated deposit. The peculiar beds of stratified sands and gravels on the low plains already referred to, and which prove the former presence of the sea, are found at elevations of from 200 to 260 feet along the margins of the hills, and against or upon the unmodified bowlder-drift (Fig. 2). Beach-sands occur at 230 feet elevation, having the well-known structure of such beds. We have, too, the further fact, already noticed, that the tops of some of the highest hills of our island are composed of stratified gravel and sand.

Without insisting further on this fact, however, we think a movement of subsidence is shown thus far of at least 260 feet; but facts of a most interesting and important character now being brought to light show that this is but part of the great movement of depression

of the period we are considering. An artesian well is being bored on Barnum's Island, already mentioned, within two miles of the ocean. The island is but a knoll of tillable upland, surrounded by meadows and the waters of the bay.

The boring has reached a depth of 368 feet, or about 358 feet below

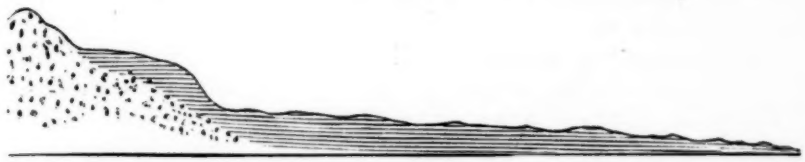

FIG. 2.—SECTION FROM HILLS AT CENTRE OF THE ISLAND, NEAR WHEATLY, TO THE OCEAN, SHOWING STRATIFIED BEDS OVERLYING THE UNMODIFIED DRIFT.
Vertical section, one inch to five hundred feet. Horizontal section, one inch to three miles.

tide-level, and is still in progress. We present below a statement of the series of deposits penetrated. Our table is prepared from the record of Theodore F. Carman, Esq., of Hempstead, Long Island, engineer of the work, and from eighty specimens of the layers passed through, furnished by that gentleman:

| | | |
|---|---|---|
| 1.— | 70 feet. | Yellowish gravel and sand. |
| 2.— | 56 " | Clay. Upper portions with decayed vegetation, wood, and lignite. |
| 3.— | 3 " | Coarse gravel and sand. |
| 4.— | 46 " | Sands, with clayey crusts and pyrites. |
| 5.— | 25 " | Sands, with some lignite. |
| 6.— | 54 " | Clayey sands. |
| 7.— | 94 " | Fine sand, sandy clay, with much lignite in the more clayey portions. |
| 8.— | 4 " | Fine sand. |
| 9.— | 1 " | Very firm bed of lignite, in fine decayed vegetable matter and clay. |
| 10.— | 10 " | Clay, with lignite. |
| 11.— | 3 " | Clay, very fine, without lignite. |
| 12.— | 2 " | Sandy clay. |
| Total, | 368 | |

Our grouping of the deposits may suggest transitions more sharp than the specimens warrant. About 70 feet of the surface is of yellow or orange colored sand and gravel. Then occurs a bed of clay 56 feet thick, on the surface of which was found decayed vegetable matter having a strong odor of carbolic acid. Wood and lignite occurred in this bed. Beneath was found a layer of coarse gravel and sand.

None of the layers penetrated are of the unmodified drift. No shells have been found, nor other remains of animal life, excepting a very small fragment of a crinoidal stem which occurred in a thick bed of coarse silicious sand 160 feet below the surface. This specimen, which is much water-worn, Prof. Dana suggests may be of cretaceous species, but it affords little or no evidence that the deposit in which it was found is of Cretaceous age. The Silurian and Devonian fossils, which occur frequently in the drift of Long Island, cannot be considered as proof that the deposits are Silurian or Devonian.

With the specimens before us, we think these surface-beds, to a depth of about 180 feet, are post-glacial, and are formed from glacial drift. Below this depth the beds are of dark clay and clayey silt-like sand, alternating with deposits of sand similar to that of the beaches upon the coast. Lignite occurs throughout, and a layer of it at 353 feet was penetrated with difficulty by the implements employed in boring the well.

The lignite found throws little light on the age of the beds. It is brought to the surface in small pieces, and that from the surface of the clay bed at 353 feet was formed from small stems of exogenous structure. The same is true of that found on the bed at 70 feet. This deposit of clay, 56 feet in thickness, seems closely analogous to many clays now upon, and at various depths beneath, the surface of the island. It is evidently a local deposit, such as might occur in a depression of the surface. Two tube-wells have been driven at no great distance from Barnum's Island, one 97, the other 194 feet, in which no similar layer of clay was detected. No green sand or marl deposits have been found. It seems probable that the beds below a depth of 180 feet were formed in tranquil waters of no great depth, possibly in an estuary sheltered by a beach of sand from the waves, into which streams discharged the waste of what was evidently a forested region. That they are preglacial, is, we think, quite certain. The period of transition between the Tertiary and the Drift, when the distant but advancing ice-sheet sent on its swollen streams, best answers the conditions. The existence of beds of stratified gravel and sand at 260 feet above the level of the ocean, and of similar beds at 180 feet below it, which we refer to the period of the Champlain subsidence, proves that a depression took place of at least 440 feet, and further that the coast was at least 180 feet higher than now when the depression began. That the elevation was much greater than that will be obvious as we proceed.

The glacial drift of Long Island, of which the Champlain deposits were formed, is without fossils, excepting such as occur in bowlders from older beds. But underneath it is a deep deposit of sands, gravels, and clayey sands, in which fossils have been found. Shells of the clam and oyster were taken from sands beneath the bowlder-drift at Lakeville, at a depth of 140 feet, by Henry Onderdonk, Jr., Esq., and a mass of shells, chiefly oyster, were found in sinking the well of the Nassau Gas-Light Company in Brooklyn, 127 feet below the surface, beneath a layer of unmodified drift, 70 feet thick. In this drift were many bowlders. A section of this well is shown in Fig. 3.

In digging wells these sandy and gravelly layers are generally found beneath the bowlder-drift; and the shells, wood, and lignite, evidently represent a period of milder climate than prevailed during the Ice period. Possibly they were deposited on the coast by the floods and swollen streams of the approaching glacier, and the coarse-

ness of the upper portions may suggest a shoaling of the coast from an upward movement of the land as the Ice period came on.

That an elevation occurred during the progress of the Ice period is evident from the contour, as well as structure, of the drift of Long Island. On the north side of the island are numerous fiord valleys, constituting a series of harbors of unsurpassed beauty. They extend

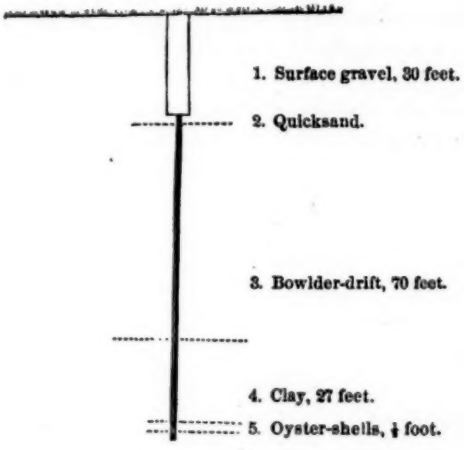


FIG. 3.—SECTION OF THE NASSAU GAS-LIGHT COMPANY'S WELL IN BROOKLYN.

into the island from two to six miles, having depths of water from ten to thirty, and in some instances fifty feet. Beneath the water is a deep deposit of ooze or sediment, known to be in one case forty feet thick. The banks on either side are from a few feet to 200 feet high.

It is apparent that these enormous valleys were not wholly cut into the drift after it was deposited, but rather were maintained while the deposit of drift was in progress, as valleys or water-courses, through which glacial streams may have been discharged into the ocean. These became filled, however, by an excessive accumulation of drift, as from rapid wasting of the ice, causing the outflowing streams to be arrested, and the waters to be discharged eastward or westward from Long Island Sound. But whether they were formed as we suggest, or were cut into the drift after it was deposited, it is quite certain that the coast was sufficiently elevated to permit the glacial floods to sweep the bottom of those valleys.

The 70 feet of drift of the Nassau Gas-Light Company's well is wholly below tide, and its unmodified structure shows that it was deposited above the sea-level, or out of the reach of waves, and further confirms the elevation of the coast in the glacial period.

But evidence of the elevation of the coast during the progress of

the Ice age is shown in the interesting fact revealed by the United States Coast Survey, and noticed by Prof. Dana, that the old valley of the Hudson River exists a well-defined depression in the bed of the ocean through a distance of 89 miles southeastward from Sandy Hook. It is termed on the Coast Survey chart "a remarkable gorge." The soundings show that it comprises a continuous series of deep depressions in the ocean's bottom. Some of these are eight miles long and from one and a half to two and a half miles wide. The map (Fig. 4), kindly furnished by the publishers of Prof. Dana's "Manual of Geology," shows how the dotted lines of equal depths bend toward Sandy Hook, indicating the line of deepest water, or the old river-valley in question.

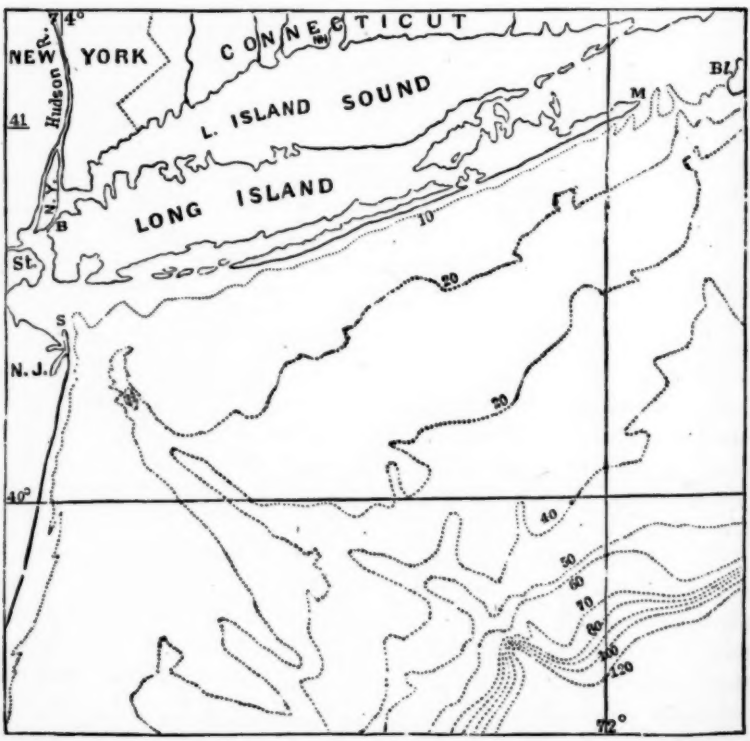


FIG. 4.—MAP OF THE SUBMERGED BORDER OF THE CONTINENT OFF LONG ISLAND AND NEW JERSEY, WITH LINES OF EQUAL SOUNDINGS IN FATHOMS. (From Dana's "Manual of Geology," p. 422.)

The Coast Survey chart shows us that at 28 miles from Sandy Hook the depression is 90 feet below the banks or ocean-bottom on either side. At 39 miles and at 51 miles the depression is 60 feet, and at 74 miles it is 72 feet, and at 89 miles it is 492 feet.

The average depth of the ocean over this depression is about one-third greater than on either side.

Through this valley, probably much deeper than now, during the period of elevation the Hudson flowed on its way to the ocean. Had it been, as it now is, deeply submerged, no river could have flowed in it, nor could it have been maintained as a valley while the deposit of drift was going on. The facts imply elevation of the region to an extent of from 300 to 400 feet above the present level of the sea. This would change in a marked manner the aspect of the coast. The site of the city of New York would be inland and greatly elevated, while the gorges of the Hudson and East Rivers would be deepened and widened by glacial torrents and ice. The ocean-border would be from 70 to 80 miles southward from the present shore of Long Island, and the deepest point attained in the artesian well on Barnum's Island would be above the level of the sea.

There is reason to conclude that the entire subsidence of our coast, from its greatest elevation in the glacial age to the greatest depression of the Champlain period which followed it, was from 600 to 700 feet, possibly much more than that. The elevation which followed carried its stratified deposits not only to their present height above tide, which, as we have seen, is about 260 feet, but at least 63 feet more than that when the buried marsh at Fort Lafayette was formed at the surface. By the present subsidence it is submerged or buried to that depth.

Here we pause. Further observations may confirm or correct our conclusions. Geology has not a more tangled skein than is presented in the structure of Long Island. There is evidence of minor oscillations, and pauses of movement, during which great clay deposits formed in depressions upon the surface, now deeply covered with drift or stratified sands, affording also some evidence that interglacial periods, perhaps of mild climate, occurred, but more observations and more facts are needed to justify a definite judgment on the subject.

Underneath the glacial drift, and underneath the sands which we refer to the advent of the ice, are beds of clay and colored sands which appear to be independent of the drift, and are referred to Tertiary or Cretaceous periods. They appear at the surface along the north side of the island, and are found buried by both unmodified drift and by coarse glacial rubble.

We present in tabular form the series of deposits which seem well defined on Long Island, and which represent the probable order of events, but they are fragmental, and perhaps do not occur anywhere in a continuous vertical series:

1. Shore and other surface formations.
2. Stratified gravels and sands of the Champlain subsidence—with fossil shells of clam, oyster, and scallop; also wood and lignite.
3. Coarse glacial rubble in deep beds without fossils, representing floods at the close of the Ice period, chiefly on the north side of the island.
4. Unmodified bowlder-drift without fossils.

5. Stratified gravels and sands with fossil shells of the oyster, also wood and lignite underneath the bowlder-drift.

6. Laminated sands and clays with decayed vegetation and lignite have been found; also one shark's tooth (*C. angustidens*).

7. The above beds seem to merge into more clayey sands, and in the deeper portions fine dark-colored plastic clay.

The period we have considered is one of immense duration, but throughout there is no evidence of sudden or violent changes. No catastrophe has "set Long Island off from the mainland." In its wonderfully complex structure it is a monument of a state of things which has passed away, but also of a series of movements of oscillation which has continued to the present time.

But it is only after long periods of time that these become obvious, and we realize how completely old landmarks have disappeared.

The tourist in Italy lingers with astonishment before the erect columns of the temple at Pozzuoli in the bay of Baiæ, and sees, at a height of twenty feet above their base, proof of their long submergence in the waters. Moore said of them:

> "These lonely columns stand sublime,
> Flinging their shadows from on high,
> Like dials which the wizard Time
> Has raised to count his ages by."

But, on our own shores, beneath the clear waters, and on the hills we cultivate, are records of similar movements, vastly greater in extent, and running with marvelous continuity through periods so vast that all the centuries which have passed since the Pozzuoli marbles were erected seem but as yesterday.

## AN AMERICAN ASTRONOMICAL ACHIEVEMENT.

By RICHARD A. PROCTOR.

AN American astronomer, Prof. Young, of Dartmouth College, Hanover, New Hampshire, has recently achieved a victory over a problem which has for many years foiled the skill of the best European observers; and, in so doing, he may be said to have added the keystone to an arch of no small importance in the edifice of modern astronomical science. It will be in the knowledge of most of my readers that astronomers have succeeded, during the last eight years, in measuring the rate at which some of the stars travel from or toward us, employing for the purpose what is called the spectroscopic method. I do not mean here spectroscopic analysis simply, but a special application of this now familiar analysis to measure the rate at which luminous bodies are approaching us or receding from us.

The principle of the method is very readily explained. Light comes to us from the heavenly bodies, as from other luminous bodies, in waves, which sweep through the ether of space at the rate of about 185,000 miles per second. The whole of that region over which astronomers have extended their survey, and doubtless a region many millions of millions of times more extended, may be compared to a wave-tossed sea, only that instead of a wave-tossed surface there is wave-tossed space. At every point, through every point, along every line, athwart every line, myriads of light-waves are at all times rushing with the inconceivable velocity just mentioned. It is from such waves that we have learned all we know about the universe outside our own earth. They bring to our shores news from other worlds, though the news is not always easy to decipher.

All the celestial bodies are in motion amid the multitudinous waves of space. Something can be learned respecting their motions by studying the waves. If a strong swimmer were stemming a series of long, rolling waves, their crests would pass him in more rapid succession than if he were at rest; if, on the other hand, he reversed his course, so that waves overtook instead of meeting him, their crests would pass in slower succession. One can easily conceive how, if he knew the exact rate at which the crests would pass him—so many exactly per minute—were he at rest, their slower or more rapid succession might indicate how fast he himself was moving, either from or toward them. If he were quite unconscious of his own motion, the effect would be simply that the distance from crest to crest would seem to be diminished in one case, lengthened in the other—that is, the waves narrowed or widened. Similarly with the aërial waves which produce sound. They are seemingly shortened when the source of sound is approaching, whether by its own motion or the hearer's, and lengthened when the source of sound is receding. In the former case the tone of the sound is made more acute, in the latter graver than it really is. This is strikingly illustrated when a swift train rushes past a station, the whistle sounding all the time, for there is a perceptible lowering of the whistle's note as the engine passes a hearer on the platform. While the train is appproaching him, he hears a note somewhat sharper than the true note of the whistle; after it has passed he hears a note somewhat flatter than the true note. Still more obvious, even to non-musical ears, is the corresponding change when two trains pass each other. In America, where a hideously-clanging engine-bell is used, the change is very remarkable, insomuch that a person unfamiliar with the arrangement actually adopted would suppose a different bell was rung the moment the engine passed the hearer.

Light traveling also in waves, it is obvious that a similar effect must be produced by approach or recession, if only the rate of motion is sufficiently rapid. The swimmer of my first illustration must have

a velocity comparable with that of the water-waves, or no change will be observed. The trains of the second illustration must have a velocity comparable with that of the aërial waves producing sound, or no change of tone will be produced. And in like manner a star or other celestial body must be approaching us, or receding from us, with a velocity comparable with that of the ethereal waves producing light, or no change of color will be produced, the color of light corresponding with the tone of sound. Unfortunately for this purpose, though most fortunately in other respects, light travels at so enormous a rate that even the swiftest motions of the heavenly bodies seem rest by comparison. What, for instance, is the rush of even Newton's comet past its point of nearest approach to the sun, though at the rate of more than 300 miles per second, to the flight of light over nearly 200,000 miles in the same time? Very much as the movement of a person taking only six steps a minute, each less than half a yard long, to the rush of the swiftest express-train. Yet astronomers have undertaken to measure the approach and recession of stars, moving—in some cases—with less than a tenth part of that comet's motion, and whose velocity, therefore, sinks into still more utter insignificance by comparison with that of light.

Secchi claims (but not justly) to have first invented and applied the method used for this purpose, which consists in noting whether some known line of the spectrum of a heavenly body changes in position—either by moving toward the violet end of the spectrum, which would imply approach, or by moving toward the red end, which would imply recession. Of course, the method is exceedingly delicate and difficult, involving a number of details which would be quite unsuited to these pages; but that, in principle, is its nature. Secchi tried the method, and failed to get any results from it, announcing his unsuccessful attempt in March, 1868. "Then," he says, "Mr. Huggins retried (*reprit*) the method, announcing in April, 1868, the discovery that Sirius is receding at the rate of twenty miles per second." Secchi should know well, however, that our great spectroscopist did not achieve this success in the few weeks between Secchi's announcement of failure and Huggins's announcement of success. Months had elapsed, during which Huggins had been struggling with this difficult problem. If the enunciation of the method gave claim to the credit of its successful application, I myself could advance a stronger claim than Secchi's, for in an essay in *Fraser's Magazine* for January, 1868, I definitely indicated the nature and value of the method. But I would rather refer to the circumstance as enabling me to support Huggins's assertion that he was observing by this method for months before Secchi announced his own failure, for immediately on the appearance of my essay I received a letter from Dr. Huggins, mentioning (in confidence, until his paper should be published) that he had been for some time striving for success

by the method I had described. This was nearly three months before Secchi's paper appeared. Subsequently Huggins observed by this method a number of stars, some of which he found to be receding from us, others approaching us.

Recently, however, the method itself has been called in question —first, by Van der Willigen, for reasons professedly mathematical, but unsound; secondly, by Secchi, because of his failure to see what Huggins has observed. Secchi had once based his attack on his failure to detect by this method the effects of the sun's rotation. As the sun's equator is spinning swiftly round, it is necessarily approaching on one side and receding on the other. By some amazing miscalculation, never yet explained, Secchi made the rate of this motion many times larger than it really is—so large, in fact, that the method we have described should have exhibited the sun's whirling motion. Small as the effect really is—amounting, in fact, only to a relative motion of about two and a half miles per second—Huggins did not despair of recognizing it; but he failed, though he used a double and twice-acting battery of prisms (the invention of the present writer—so far, at least, as its duplex character was concerned) made by Mr. Browning. Mr. Christie, of Greenwich, after resolutely grappling with the work of determining star-motions by this method, and in the main confirming Huggins's results, succeeded in recognizing by its means the known motions of Venus toward or from the earth, in various parts of their respective orbits. This was a great triumph, and more than met Secchi's objections. But Prof. Young has gone, for the present, ahead of all other observers by this method. Availing himself of a beautiful extension of spectroscopic powers, due to Dr. Rutherfurd, of New York, he has succeeded in unmistakably recognizing the effects of the sun's motion of rotation by the spectroscopic method. Young has made the observations so satisfactorily that he relies even upon the difference between his results and the measured rate of the sun's rotation. He finds the sun's atmosphere (whence, of course, the spectral lines come) to be traveling faster than the sun's visible surface. To use his own words, "The solar atmosphere really sweeps forward over the underlying surface, in the same way that the equatorial regions outstrip the other parts of the sun's surface." The difference of rate is about ten miles per minute. For my own part, I doubt very much whether so small a difference can be indicated by this method. But even if we regard this part of Young's work as not yet proved—nay, even if we go further, and accept nothing more than the bare recognition of the sun's rotation by the new method—he must be congratulated on having effected the most delicate piece of spectroscopic observation yet achieved by man. He has placed beyond doubt or cavil a method of motion-measuring the most remarkable yet invented, and likely, as instrumental means improve, to be most fruitful in results of astronomical interest and importance.

# NATURE AND LIFE IN LAPLAND.[1]

By C. CHAMBERLAIN.

NONE who have had experience of travel in Swedish Lapland are likely to deny to it the charms of perfect freshness and originality. The almost primitive character and habits of the people, the singular conditions of their life, the unique splendor of the scenery, the bright intoxication of the air, and the glory of the arctic sunsets, are all a constant source of pleasure and surprise. For the angler there is almost unlimited trout and grayling fishing, with possibilities of salmon; and for the sportsman abundance of ptarmigan, willow grouse, hares, and wild-fowl of all descriptions; while the cost of living, not indeed sumptuously, but sufficiently well, may be covered by two or three shillings a day. Unfortunately, these advantages can only be reached by routes so little tempting to the ordinary tourist that it appears from the visitor's book at Quickjock that only three hundred persons in twenty years have braved the discomforts of the approach. Now, however, that Norway is becoming hackneyed ground, and that all its available streams are rented and preserved, it is possible that the attractions of Lapland may yet counterbalance the well-founded objections to the gulf of Bothnia. At the present time the trip cannot be recommended to ladies, unless they are willing to put up with more than the usual inconvenience and discomfort of out-of-the-way travel; but for men, willing to rough it a little, there is no hardship or difficulty greater than those with which most sportsmen must be already familiar.

Stockholm, the starting-point of the expedition, may be reached direct by Hull and Gothenburg; or, if the land-route be preferred, through Calais, Cologne, and Hamburg, and thence, either through Jutland to Friedrickshavn, and across the Cattegat to Gothenburg, or by Kiel and Korsoer to Copenhagen, and thence by Malmö to Stockholm. For bad sailors the last route is to be preferred, as in the other cases the traveler must make the acquaintance of either the Skaggerack or the Cattegat, or of both; and he will probably find that their names are not rougher than their waters, and that they are in fact the most diabolical cross-seas on the face of the globe. The captain of the little steamer which plies between Gothenburg and Friedrickshavn, who has spent the greater portion of his life in ocean-ships, informed us that he never dared to go below when the Cattegat was rough, but found his only safety from sickness in the fresh breeze on deck.

[1] From an article entitled "A Visit to Lapland, with Notes on Swedish Licensing," *Fortnightly Review*, December, 1876.

The distinctive beauty of Stockholm is in its situation. Built partly on islands in Lake Malar, it is intersected in every direction by the waters of the lake and of the Baltic, and, with its busy quays, broad streets, handsome buildings, pleasant gardens, and clear atmosphere, is certainly one of the brightest and most charming capitals in Europe. The streets are still enlivened by the gay costumes of the peasants, especially those of the nearest provinces; it is said, however, that their use is gradually dying out before the advance of railroads and other enemies of the picturesque.

The Swedes are undoubtedly a fine race; many of the men are very tall, and the women are almost universally refined-looking and graceful in their carriage. A crowd of Swedes might at any time be mistaken by an Englishman for a crowd of the better sort in his own country; and in character there is the same resemblance to a high average English standard. The middle and trading classes have great sympathy with the English nation and its institutions, and are ready at all times to express and prove it; the aristocracy and higher ranks of society are more inclined to favor French manners and customs, but this is due to the influence of the court and to the origin of the royal family. Every educated Swede reads and probably speaks English well, and with very slight, if any, foreign accent. English newspapers and books of all kinds are largely read, and English literature is a prominent branch of study at the high or middle-class schools, of which, as of all other educational institutions, there is an ample supply in Sweden. All along the coast of the gulf of Bothnia, in every little town of a few hundred, or at most of two or three thousand, inhabitants, there is a large school of this description, with a full staff of masters, *lektors*, and assistants, provided according to a fixed scale, and forming part of the general organization for national instruction. We met several of these teachers, and found them extremely well-informed and intelligent men, speaking English, French, and German, and accepting for the communication of these acquirements salaries which would be deemed totally inadequate in any other and richer country. They were all home-taught, by books and not *viva voce*, and hence, though well qualified to translate English into Swedish, they found it more difficult to reverse the process and to interpret their thoughts into elegant English. "The weather is deplorable," said one of these gentlemen; "it makes for the melancholy, and influences on the humors."

The fees charged in the schools are moderate, and such as to induce a general acceptance of the educational advantages offered by the class for whom they are intended. Primary education in Sweden is free and compulsory, though it is seldom necessary to recur to the interference of the magistrates. The Swedes cannot be made to understand the beauty of our English system, by which a national service, undertaken on the distinct ground of its importance to the

whole community, is made unpopular by a charge extorted from the persons whose ready and voluntary acceptance of the service is the object desired. They argue that the state, as a whole, is bound to secure to all its citizens the opportunity of acquiring at least the elementary knowledge which is requisite for its security and general well-being, and that it is the function of the state to offer this instruction free of charge before it attempts to compel any individual to avail himself of it. They attribute the almost universal prevalence of primary instruction in their country to the existence of these free schools, and point to their wide popularity as sufficient evidence of the fallacy of the proposition, so often taken for granted in England, that the poor do not value education which is paid for out of the general taxation of the community.

Steamers leave Stockholm for Haparanda, at the head of the gulf of Bothnia, two or three times a week, calling on the way at the ports on the west coast. Against a head-wind these boats roll and pitch in an extremely provoking fashion; but, during the summer months, the voyage is generally a smooth one. The boats carry stores to the towns on the route, and bring back tar, which, with wood, and iron from the mines of the great Gellivara Company—now the sole property of an English merchant—constitute the chief trade of the gulf. The coast navigation is extremely intricate and difficult, the steamer winding its way for hours through the fiords and among innumerable rocky islets. On one occasion we bumped over a sunken rock, and, if one may judge by the composure of the captain, this must be no infrequent occurrence, though it smashed all the crockery laid out in the saloon and greatly alarmed the passengers. At night, and on the occasion of a fog, progress is impossible, and the steamer is brought-to and anchored till daylight or clear weather.

Our destination was Luleä, which is reached in about seventy-two hours from Stockholm, and is a town of some 2,000 inhabitants, situated at the mouth of the great river of the same name. The harbor, after the difficulties of the entrance are surmounted, is a fine one, and many English and other ships lie here, loading timber; it is floated down the river from the forests, and cut into planks or made up into frames for doors and windows at the saw-mills in the town and neighborhood.

The houses are almost entirely built of wood, and are in many cases shops and warehouses, as well as dwelling-houses, although there is little display of goods in the windows. There is a large school, attended by the youths from all the surrounding district, as well as by those resident in the town itself. Luleä is the seat of the government of the province of Norbotten, which includes the whole of Lapland, and has a population of 80,000, scattered over 1,932 square miles of country. The governor, who has no sinecure, being required to visit personally his immense district several times a year,

is provided with an official residence and a salary of 12,000 Swedish crowns, or about £650 per annum.

On arriving at the inn, which is good and clean, and makes up some forty beds, one is struck with a peculiarity of all similar places in Sweden, namely, the apparent indifference to visitors exhibited by the proprietor. No head-waiter, with attendant circle of porters and chambermaids, awaits the arrival of the guest. The luggage is put down at the entrance, and the traveler must seek for himself his rooms and the information he requires; while the landlord, with his hands in his pockets, regards his efforts from a window with languid curiosity. There is no intentional incivility, but it appears not to be the custom to welcome the coming guest, although to speed the parting guest there is abundance of hand-shaking and hearty good wishes. The curious custom of the Smörgos prevails at these inns, and indeed everywhere throughout Sweden; it consists in a standing refreshment provided at a side-table free of charge, and comprising bread and butter, cheese, caviare, dried fish and reindeer-flesh, sausages, and other similar delicacies, to be taken immediately before each regular meal, and washed down with *branvin* and other neat spirits. In connection with this performance the Swedes have an objectionable habit, which may be called the community of forks, as the same implement passes rapidly from mouth to mouth and from dish to dish; the rights of private property are flagrantly disregarded.

From Luleä a succession of three small steamers, each making its passage to the bottom of considerable rapids, carry the traveler some ninety miles up the Luleä River to its junction with the Little Luleä at Storbachen, and across the frontier of Sweden into Lapland, which commences about ten miles below the confluence. The scenery is extremely striking, especially toward the end of the road. The river is a noble stream, never narrower than the Thames at Westminster, and expanding at intervals into broad stretches of water which, shut in by the windings of the river, present the appearance of considerable lakes. The banks are lined with the pine-forests for many miles, and the dark green of the firs and larches is varied by the brighter foliage and silver bark of the birches, which grow in considerable numbers among the other trees. At intervals, gradually getting longer as the distance from Luleä increases, the villages or settlements of the Swedish farmers break the uniformity of the scene, and the wooden houses and out-buildings, painted bright red, with the windows and doors picked out in white, and surrounded by small clearings with patches of yellow barley and green pasture, stand out brightly against the sombre background of the forests, and give animation and warmth to the landscape. It is difficult to convey the peculiar fascination of this scenery. It is due especially to the sharpness and contrast of color, the bright clear blue of the sky giving definiteness to the outlines of the trees and hills, and bringing

into marked relief all the incidents of the view. There is something bracing in the very appearance of the landscape, to which the noble river is an ever-fitting foreground.

At Storbachen the river has to be exchanged for the road, and a country cart holding two persons, and with or without an apology for springs as chance may determine, carries the tourist along the banks of the Little Luleä to Jockmock, a distance of some thirty miles. This drive is in itself a unique experience. The road after wet weather is cut up into deep ruts, in and out of which the cart plunges with a violence most discomforting to its occupants, who are bruised and pounded without the possibility of resistance. It must be admitted that the process detracts from the pleasure of the excursion, which in other respects is extremely interesting. The route lies for the whole day through the almost trackless forests. Hardly a human being is to be met in these immense solitudes, and the silence is only broken occasionally by the note of some strange bird or the movement of the wind through the trees. In many places forest-fires have ravaged the country for great distances, and everywhere there is a vista of blackened stems or falling trunks. In contrast to this desolation, where the fire has not passed, the ground is carpeted with the most luxuriant mosses and lichens in all the tints of green and red and yellow, while an occasional clearing, though at very rare intervals, relieves from time to time a sense of utter loneliness by the evidence it gives of the neighborhood of human beings.

The forests cover nearly one-half of the whole surface of Sweden, and constitute an important part of the wealth of the country and the revenue of the Government. In past times they were very carelessly managed, and in many cases were sold outright and without conditions to merchants, who ruthlessly cut down the timber with sole regard to their immediate interests. The pine is of very slow growth, increasing only one inch in diameter in ten years, and reaching twelve to fourteen inches in a century; and the wholesale destruction of young wood has left large tracts desolate and unprofitable for an indefinite period. The soil is excessively poor, consisting of sand with the thinnest possible coating of vegetable mould, so that no ordinary cultivation is possible.

Now the forests are strictly looked after, and no land is sold; but the right of cutting wood, limited to trees of ten inches and upward in diameter, is let for a term of years and by tender, at so much per tree. In the remote districts the royalty is about 1*s.* 3*d.* per tree, and the lessees have in addition to carry out works for deepening the rivers and keeping them clear of all obstructions. Twenty years ago the value of trees on the ground was not more than threepence or fourpence apiece.

From Jockmock to the end of the journey at Quickjock the mode of traveling and the scenery are again changed. The head-waters of

the Little Luleä are a series of large lakes, from six to thirty miles long, and varying in breadth from two miles to seven or eight. These in turn are fed by two mountain-rivers, which join their floods at Quickjock, and pour the united stream into the uppermost lake. They are traversed in long, open boats made of very thin wood, and rowed by two or three men, according to the weight of luggage and the length of the journey. These boats are unprovided with seats, and the passengers have to squat at the bottom back to back, or crowded side by side; and, as very little movement would be sufficient to swamp so frail a craft, the limbs get cramped and stiffened, and the journey becomes very fatiguing. With a high wind the broadest lakes become rough and dangerous, and on one occasion we shipped so much water that it seemed doubtful whether our expedition would not come to an untimely end. Each lake is connected with the next by strong rapids, in some cases rising into small waterfalls, and to avoid these it is necessary to disembark, when the luggage is carried on the shoulders of the rowers through the pine-forests to the next lake. Throughout this part of the trip the silence can almost be felt, and becomes at last oppressive. No living thing is seen for hours except occasional flights of wild birds, or a solitary heron disturbed by the passage of the boat. Hills, gradually developing into mountains, and finally covered with snow as the neighborhood of Quickjock is reached, shut in the scene, and the slopes of these are covered almost entirely with stunted pine, the birch having nearly disappeared. There is, however, no lack of color, as the firs in the sunlight present many shades of the darker greens intermingled with a rich brown where some disease appears to have attacked the trees. A large sweep of pine-forest thus spread out in an amphitheatre of hills, and seen from a great distance, might be mistaken for an expanse of heather and fern, browned by the autumn rains and sun, though of course the brighter purples are absent from the Lapland view.

In the summer months there is perpetual daylight in all these regions, and the midnight sun is visible for some time in June. When we were there, in September, it was light till nine or ten o'clock, and never absolutely dark. The sunsets were most gorgeous, dark masses of purple clouds being lit up with the intensest hues of gold and crimson as the sun went down behind them, a glowing ball of fire. On one occasion the effect was heightened by the appearance of the eastern sky, which shaded off from deepest rose at the zenith, through delicate gradations of pinks and purples, into a lovely pale, pure blue, in the midst of which the full autumnal moon shone gloriously.

The fishing in the lakes is exceedingly good, and very large trout, and even salmon, may be caught with the minnow and other spinning bait. For fly-fishing the best places are the rapids between the lakes, through which the boat is screwed in and out in an extremely clever and dexterous way by the boatman, who takes advantage of the shel-

ter of every rock and stone as he passes from one to the other, while the stream shoots by. In favorable weather an angler may easily land a hundred-weight of trout and grayling in a day's sport, the fish running from half a pound to two pounds in weight. The flies sold by the London makers should be supplemented by some of a smaller size for bright weather and clear water; one with a body of yellow silk and grayish-brown wings is said to be very killing.

The distance from Jockmock to Quickjock, the two principal villages on the route, is about ninety miles, and is performed in three days. Each of these places has a church, a school, and a post-office, and Jockmock is said to have a shop, though we could not find it. They are really collections of small wooden huts, vacant during the summer months, but occupied in the long winter by the Lapps, who then come down from the mountains with their reindeer. Quickjock especially is in a delightful situation, facing a beautiful lake, and sheltered by mountains of noble outlines and grand proportions. At Jockmock there are some fine falls, not unlike the Rheinfalls at Schaffhausen, though in a very different setting. The resting-places or stations between these two villages are not inns in the usual sense of the word, but the houses of the Swedish settlers or immigrants into Lapland, one of which at each settlement is destined for the reception of the occasional guests.

These settlements consist of two or perhaps four houses, with the necessary out-buildings, and seem generally inhabited by the several members of the same family. Some of them have existed a considerable time, and are occupied now by the grandchildren or great-grandchildren of the original settlers. Originally the Government granted free gifts of land, but they have now ceased to do this, and the number of the settlers does not appear to be receiving many additions from outside. The houses usually consist of two or more large rooms on the ground-floor with lofts above, and vast chimney-hearths in one corner, in which the logs of pine, some two or three feet in length, are piled upright when a fire is wanted; being lit, they burn up in a few minutes into a roaring fire which gives out an intense heat. The family live chiefly in the kitchen, and this and the guest-chamber are about twenty or thirty feet square, and furnished with a kind of sofa-bedstead which pulls out so as to afford a sleeping accommodation of about five feet six inches by three feet. The kitchen itself is not over-clean, nor are the personal habits of the people without reproach in this respect; yet the guest-chamber, the linen, and the crockery, leave nothing to be desired.

The houses are surrounded by a small clearing, where the settlers cultivate for their own consumption sufficient oats and other grain, hay, and potatoes. They sow their corn in June, and so rapid is the growth under the influence of the lengthened days that they reap the harvest in six or seven weeks afterward, and sometimes get two crops

in their short season. The cultivation is restricted to the actual wants of the settlement, as the difficulty of transit precludes the possibility of a market for the surplus. Cattle and ponies, and sometimes sheep and poultry, are kept at each station, but the food of the family is limited to fish—which is dried for winter use—milk, black or rather brown flat bread, and dried reinflesh, with an occasional change in the shape of game or wild-fowl killed on the hills or lakes. Everywhere, even in the poorest houses, the most excellent coffee is obtainable; the green berries being roasted over the fire and ground whenever a cupful or more is wanted.

In the winter, when the lakes and rivers are all frozen, and the ground is covered three or four feet deep with hard snow, the settlers go long distances on snow-shoes and in sledges, and bring up from Luleä what stores they may require. The money for such purchases is gained by winter labor in the forests, where the trees are felled and dragged to the water's edge, to be thrown in and floated down to Luleä when the ice breaks up. At this work a team of one horse and two men can earn about 40*s.* a week, which is considered large wages in this part of the world. The legal tariff for a boat in summer is one kronor (1*s.* 1½*d.* English) for each man for seven miles, with no allowance for back fare; and a small *dricks penningar*, or *pour-boire*, added to this will make them supremely grateful, and insure the generous donor many hearty shakes of the hand.

The settlers cannot afford to be ill, as the nearest doctor lives at Luleä, almost a week's journey from Quickjock. In ordinary cases they depend on their own resources, but in any serious illness the Luleä medico is sent for and is obliged to attend, being paid a small salary of £200 a year by the Government on this condition. Midwifery is performed by women. Crimes of any kind seem to be very rare; and though every settler carries a most ugly-looking dagger-knife suspended from his belt, its use appears to be confined to purely pacific purposes. The most common offenses are against the forest regulations, and the observance of these is superintended by an officer who has his headquarters at Jockmock. On *fête* days, at this latter village, a patrol is selected by the Governor of Luleä from among the steadiest of the settlers, and to him the preservation of order is intrusted.

The men are physically a fine race, and are generally honest and industrious, with an air of independence and straightforwardness. Like the poorer Swedes elsewhere, they are greatly given to the use of tobacco in all forms; and besides smoking and chewing in the usual approved methods, they actually eat large quantities of snuff, helping themselves, as the Highlanders do, with a horn spoon from a box. The women have pleasant faces, with rather refined expression. There is a strong family resemblance among them, and the type consists in large gray eyes, brown hair, rather fair complexions, a free carriage,

and not ungraceful figure, though with full waists and large hands and feet. The older women look worn, but never have the haggish and almost brutalized look which is not uncommon in old women in other countries who have led hard, out-door lives. The general expression of countenance is somewhat pathetic, though they seem contented with their strange, solitary, and joyless life; and we could never get any of them to confess that they would care to change it, nor even to complain of what, as it appeared to us, must be the terrible monotony and hardship of the long, dark winter. In looking at these settlements and considering the nature of the life we seemed to understand more clearly the position and circumstances of the immigrants who are gradually pushing farther and farther along the shores of the great rivers of the American Continent, and carrying into the solitudes of the immense forests of the West the proofs of Anglo-Saxon courage, endurance, and pertinacity.

At some of the stations we saw specimens of the original inhabitants of the lands within the arctic circle, in the persons of Lapp men and women of uncertain age, about four feet high, and dressed in skins, with blue conical caps on their heads. In Norway it is said that the Lapps are looked upon and treated as an inferior race, the pariahs of the North; but in Swedish Lapland there is no appearance of such distinctions. The comfort and even safety of the settlers depend so much on their good relations with their neighbors that they have remained on terms of equality and friendship. Intermarriages are not uncommon, and many of the present settlers show signs of the mixture of the races.

The population of Swedish Lapland is said to include 4,000 persons of true Lapp race, and in some districts this number is increasing. The children born in the mountains die fast, but those who remain in the villages are healthy. Provision is made for their instruction, and in common with the children of the Swedes they all learn to read and write, though, judging by the absence of books at the settlements, they reap little advantage from their instruction. The Lapps were converted to Lutheranism some hundred years ago, and are said to be strict religionists. At the present time some kind of revival is going on among them, a faint reflex of the Moody and Sankey movement in this country and America.

They depend for their living entirely upon their reindeer, which they take up into the mountains all the summer, feeding them in the villages during the winter, when the rein-moss, which is their ordinary food, is no longer obtainable in the woods. This migration is rendered necessary by the habits of the reindeer, which must be near snow to keep in health. When on their summer excursions, the Lapps live in tents made of rein-skins, lying at night round a fire in the centre, a hole being left in the roof for the passage of the smoke. Their food consists of rein-flesh, fish, and game, and they keep a pot, like

the gypsies, constantly on the fire, into which are thrown all contributions in the way of edibles, which are thus stewed down together into a thick rich soup. In the winter they move about on their snow-shoes, in the management of which they are extremely adroit, shooting down the hills and in and out of the trees with immense swiftness and precision. On these shoes they hunt down both wolves and bears when these animals, which are now getting scarce, cross their path; they kill them with their spears and knives, getting a reward of fifty kronor from the Government for each head killed. The sale of spirits is strictly prohibited in Lapland, as some years ago their immoderate use was decimating the population; but kegs of *branvin* are still occasionally smuggled across the borders, and produced on the occasion of *fêtes* and holidays. The Lapps have shrewd, almost cunning faces, and, though small in stature, possess great bodily strength and endurance. Their habits are extremely dirty, and they appear never to change their clothes till they fall to pieces.

## PHYSIOLOGY OF MIND-READING.

By GEORGE M. BEARD, M. D.

IN the history of science, and notably in the history of physiology and medicine, it has often happened that the ignorant and obscure have stumbled upon facts and phenomena which, though wrongly interpreted by themselves, yet, when investigated and explained, have proved to be of the highest interest. The phenomena of the emotional trance, for example, had been known for ages, but not until Mesmer forced them on the scientific world, by his public exhibitions and his ill-founded theory of animal magnetism, did they receive any serious and intelligent study. Similarly the general fact that mind may so act on body as to produce involuntary and unconscious muscular motion was by no means unrecognized by physiologists, and yet not until the "mind-reading" excitement two years ago was it demonstrated that this principle could be utilized for the finding of any object or limited locality on which a subject, with whom an operator is in physical connection, concentrates his mind.

Although, as I have since ascertained, experiments of this kind had been previously performed in a quiet, limited way in private circles, and mostly by ladies, yet very few had heard of or witnessed them; they were associated in the popular mind very naturally with "mesmerism" or "animal magnetism," and by some were called "mesmeric games." The physiological explanation had never been even suggested; hence the first public exhibitions of Brown, with his brilliantly successful demonstrations of his skill in this direction, were

a new revelation to physiologists as well as to the scientific world in general.

The method of mind-reading introduced by Brown, which is but one of many methods that have been or may be used, is as follows:

The operator, usually blindfolded, firmly applies the back of the hand of the subject to be operated on against his own forehead, and with his other hand presses lightly upon the palm and fingers of the subject's hand. In this position he can detect, if sufficiently expert, the slightest movement, impulse, tremor, tension, or relaxation, in the arm of the subject. He then requests the subject *to concentrate his mind* on some locality in the room, or on some hidden object, or on some one of the letters of the alphabet suspended along the wall. The operator, blindfolded, marches sometimes very rapidly with the subject up and down the room or rooms, up and down stairways, or out-of-doors through the streets, and, when he comes near the locality on which the subject is concentrating his mind, a slight impulse or movement is communicated to his hand by the hand of the subject. This impulse is both involuntary and unconscious on the part of the subject. He is not aware, and is unwilling, at first, to believe, that he gives any such impulse; and yet it is sufficient to indicate to the expert and practised operator that he has arrived near the hidden object, and then, by a close study and careful trials in different directions, upward, downward, and at various points of the compass, he ascertains precisely the locality, and is, in many cases, as confident as though he had received verbal communication from the subject. Even though the article on which the subject concentrates his mind be very small, it can quite frequently be picked out from a large number, provided the subject be a good one, and the operator sufficiently skillful. The article is sometimes found at once, with scarcely any searching, the operator going to it directly, without hesitation, and with a celerity and precision that, at first sight, and until the physiological explanation is understood, justly astonish even the most thoughtful and skeptical.[1] These experiments, it should be added, are performed in public or private, and on subjects of unquestioned integrity, in the presence of experts, and under a combination of circumstances and conditions for the elimination of sources of error that make it necessary to rule out at once the possibility of collusion.

The alternative is, therefore, between the actual transfer of thought from subject to operator, as has been claimed, and the theory of unconscious muscular motion and relaxation on the part of the subject, the truth of which I have demonstrated by numerous experiments.

One of the gentlemen with whom I have experimented, Judge Blydenberg, who began to test his powers directly after I first called

[1] In New Haven I saw Brown, before a large audience, march off rapidly through the aisle and find at once the person on whom the subject was concentrating his mind, although there was the privilege of selecting any one out of a thousand or more present.

public attention to the subject in New Haven, claims to succeed, even with the most intellectual persons, provided they fully comply with the conditions, and honestly and persistently concentrate their minds. One fact of interest, with regard to his experiments, is the exceeding minuteness of the objects that he finds. A large number of the audience empty their pockets on the table, until it is covered with a medley of keys, knives, trinkets, and miscellaneous small objects. Out of them the subject selects a small seed a little larger than a pea, and even this the operator, after some searching, hits precisely.

One may take a large bunch of keys, throw them on the table, and he picks out the very one on which the subject concentrates his mind.

Another fact of interest in his experiments is that, if the subject thinks over a number of articles in different parts of the room, and, after some doubt and hesitation, finally selects some one, the operator will lead him, sometimes successively, to the different objects on which he has thought, and will wind up with the one that he finally selected. He also performs what is known as the "double test," which consists in taking the hand of a third party, who knows nothing of the hidden object, but who is connected with another party who does know, and who concentrates his mind upon it. The connection of these two persons is made at the wrist, and the motion is communicated from one to the other through the arms and hands. The "double test" has been regarded by some as an argument against the theory that this form of mind-reading was simply the utilizing of unconscious muscular motion on the part of the person operated upon.

This gentleman represents that the sensation of muscular thrill is very slight indeed, even with good subjects; and, in order to detect it, he directs his own mind as closely as possible to the hand of the subject.

In all these experiments, with all mind-readers the requirement for the subject to concentrate the mind on the locality agreed upon is absolute; if that condition is not fulfilled, nothing can be done, for the very excellent reason that, without such mental concentration, there will be no unconscious muscular tension or relaxation to guide the operator.

Experiments of the following kind I have made repeatedly with the above-named gentleman:

A dozen or more pins may be stuck about one inch or half an inch apart into the edge of a table: I concentrate my mind on any one of these pins, telling no one. The operator enters the room, gets the general direction of the object in the usual way (*à la Brown*), and, when he has come near to the row of pins, he will limit the physical connection to one of his index-fingers, pressing firmly against one of mine, and in this way he soon finds the head of the pin on which my mind has been concentrated. The only limitation of area in the locality that can be found by a good mind-reader with a good subject is,

that two objects should not be so near to each other that the finger of the operator strikes on both at once. When I began the study of this subject, I supposed, even after the true theory of the matter had become clear to me, that very small objects and narrow areas could not be found in this way. Subsequent experiments showed that this supposition was erroneous. In a wide hall, in the presence of a large audience, where the subject had the right to think of any object he chose, Brown once found, after considerable searching, so limited an area as a capital letter in the title of a newspaper pinned up on the wall and barely within reach. About an hour after, in the same place, he found a very small vial out of quite a large number ranged in a row. Although reasoning deductively from the known relations of mind to body, I had established conclusively to my own mind that the so-called mind-reading was really muscle-reading, yet I could not believe, until the above-named experiments had been made, and frequently repeated, that it was possible for even the most expert operator to find such small objects; and no physiologist, I am sure, would have believed such precision in these experiments conceivable until his general deductions had been many times verified, and supplemented by observations in which every source of error was guarded against.

As already remarked, there are a variety of ways of making the physical connection between subject and operator. A lady with whom I am acquainted goes out of the room, and while she is absent an object is hidden. She returns, and two ladies, who know where the object is stand up beside her in the middle of the room and place both of their hands upon her body, one hand in front, the other behind; all three stand there for a moment, the two subjects who know where the object is, keeping their minds intensely concentrated on that locality. In a moment or so this lady who is to find the object moves off in the direction where it is, the other ladies with her still keeping their hands upon her, and in nearly all cases she finds it. This is accomplished by the unconscious muscular tension of the two ladies who know where the object is, acting upon the person of the lady who is seeking it.

This experiment I have repeated with a number of amateur performers, and in all cases with pretty uniform success. This method is easier, both to learn and to practise, than some of the others; it is also far less artistic, and is not at all adapted for the finding of very small localities. It illustrates, however, the general principle of mind acting on body producing muscular tension in the direction of the locality on which the thoughts are concentrated.

The relaxation, when the locality or its neighborhood is reached, is not so distinctly appreciated in this method of experimenting, which is sufficient, however, to enable the operator to get the right direction and to proceed until the corner or side of the room is reached; then, by a combination of manipulation and guess-work, she will, after

a few trials, get hold of the precise object hidden, or locality thought of. When the operator and subject are connected by the methods practised by Brown, it is possible to detect also the relaxation when the locality is reached, and, guided by this, the master in the art knows just when and where to stop, and, in very many cases, feels absolutely sure that he is right, and with a good subject is no more liable to error than he would be to hear wrongly or imperfectly if directed by word of mouth.

The special methods of muscle-reading here described may be varied almost indefinitely, the only essential condition being, that the connection between the subject or subjects is of such a nature as to easily allow the sense of muscular tension or relaxation to be communicated. Instead of two subjects, there may be three, four, or half a dozen, or but one. With a number of subjects the chances of success are greater than with one, for the twofold reason that the united muscular tension of all will be more readily felt than that of but one, and because any single subject may be a bad one—that is one who is capable of muscular control—while among a number there will be very likely one or more good ones. For these two reasons, amateurs succeed in this latter method when they fail or succeed but imperfectly after the method of Brown.

A method frequently used, although it is not very artistic, consists in simply taking the hand of the subject and leading him directly, or, as is more likely to be the case, indirectly to the locality on which his mind is concentrated.

J. Stanley Grimes[1] thus describes the performance of a mind-reader in Chicago: "I repeatedly witnessed similar performances with different experts in this branch and under circumstances where every element of error from intentional or unintentional collusion was rigidly excluded. At the request of the company the same young lady was again sent from the room and blindfolded, as on previous occasions. The gentleman requested the company to suggest anything they desired the subject should be willed to do, thus removing any possibility of a secret agreement to deceive between the parties. It was suggested that the young lady should be brought into the room and placed in a position with her face toward the north; that the gentleman should then place his fingers upon her shoulder, as before; that she should turn immediately to the *right*, facing the south, and proceed to a certain figure in the parlor-carpet; then turning to the west, she was to approach a sofa in a remote corner of the room, from which she should remove a small tidy, which she should take to the opposite side of the room, and place it upon the head of a certain young gentleman in the company; she was then to proceed to the extreme end of the parlor, and take a coin from the right vest-pocket of a gentleman, and return to the opposite side of the room, and place the coin in the *left* vest-

[1] "Mysteries of the Head and Heart," p. 297.

pocket of another gentleman named; she was then to remove the tidy from the head of the gentleman upon whom it had been placed, and return it to the *tête-à-tête* where she originally found it.

"I must confess to no little surprise when I saw the young lady perform, with the most perfect precision, every minute detail, as above described, and with the most surprising alacrity; in fact, so quick were her motions that it was with the greatest difficulty that the gentleman could keep pace with the young lady's movements."

I have seen a performer—who, though one of the pioneers in this art, is far less skillful than many with whom I have experimented—take a hat from the head of a gentleman in a small private circle, and carry it across the room and put it on the head of another gentleman; take a book or any other object from one person to another; or go in succession to different pictures hanging on the wall, and perform other feats of a similar character, while simply taking hold of the wrist of the subject. In the experiment described by Mr. Grimes the subject placed three fingers of his right hand on the shoulder of the operator. Note the fact that in all these experiments *direction* and *locality* are all that the mind-reader finds; the quality of the object found, or indeed whether it be a movable object at all, or merely a limited locality, as a figure in the carpet or on the wall, is not known to the mind-reader until he picks it up or handles it: then if it be a small object, as a hat, a book, or coin, or tidy, he very naturally takes it and moves off with it in the direction indicated by the unconscious muscular tension of the subject, and leaves it where he is ordered by unconscious muscular relaxation. In the great excitement that attends these novel and most remarkable experiments the entranced audience fail to notice that the operator really finds nothing but *direction* and *locality*.

I have said that various errors of inference, as well as of observation, have been associated with these experiments. A young lady who had been quite successful as an amateur in this art was subjected by me to a critical analysis of her powers before a large private audience. She supposed that it was necessary for all the persons in the audience to concentrate their minds on the object as well as those whose hands were upon her. I proved by some decisive experiments, in which a comparison was made with what could be done by chance alone, that this was not necessary, and that the silent, unexpressed will of the audience had no effect on the operator, save certain nervous sensations created by the emotion of expectancy. Similarly, I proved that, when connected with the subjects by a wire, she could find nothing, although she experienced various subjective sensations, which she attributed to "magnetism," but which were familiar results of mind acting on body.

Another lady, who is quite successful in these experiments, thought it was necessary to hide keys, and supposed that "magnetism" had something to do with it. I told her that that was not probable, and

tried another object, and found that it made no difference what the object was. She supposed that it was necessary that the object should be secreted on some person. I found that this also was not necessary. She does not always succeed in finding the exact locality at once, but in some cases she goes directly to it; she very rarely fails.

In order to settle the question beyond dispute whether unconscious muscular action was the sole cause of this success in finding objects, I made the following crucial experiments with this lady: Ten letters of the alphabet were placed on a piano, the letters being written on large pieces of paper. I directed her to see how many times she would get a letter which was in the mind of one of the observers in the room correctly by chance purely, without any physical touch. She tried ten times, and got it right twice. I then had her try ten experiments with the hand of the person operated on against the forehead of the operator, the hand of the operator lightly touching against the fingers of this hand, and the person operated on concentrating her mind all the while on the object, and looking at it. In ten experiments, tried this day, with the same letters, she was successful six times. I then tried the same number of experiments with a wire, one end being attached to the head or hand of the subject, and the other end to the head or hand of the operator. The wire was about ten feet long, and was so arranged—being made fast at the middle to a chair—that no unconscious muscular motion could be communicated through it from the person on whom she was operating. She was successful but once out of ten times. Thus we see that by pure chance she was successful twice out of ten times; by utilizing unconscious muscular action in the method of Brown she was successful six times out of ten. When connected by a wire she was less successful than when she depended on pure chance without any physical connection. In order still further to confirm this, I suggested to this lady to find objects with two persons touching her body in the manner we have above described. I told these two to deceive her, concentrating their minds on the object hidden, at the same time using conscious motion toward some other part of the room. These experiments, several times repeated, showed that it was possible to deceive her, just as we had found it possible to deceive other muscle-readers.

The question whether it is possible for one to be a good muscle-reader and pretty uniformly successful, and yet not know just how the trick is done, must be answered in the affirmative. It is possible to become quite an adept in this art without suspecting, even remotely, the physiological explanation. The muscular tension necessary to guide the operator is but slight, and the sensation it produces may be very easily referred by credulous, uninformed operators to the passage of "magnetism;" and I am sure that with a number of operators on whom I have experimented this mistake is made. Some operators

declare that they cannot tell how they find the locality, that their success is to them a mystery; these declarations are made by private, amateur performers who have no motive to deceive me, and whose whole conduct during the experiments confirms their statements. Other operators speak of thrills or vibrations which they feel, auras and all sorts of indefinable sensations. These manifold symptoms are purely subjective, the result of mind acting on the body, the emotions of wonder and expectancy developing various phenomena that are attributed to "animal magnetism," "mesmerism" or "electricity"—in short, to everything but the real cause. I have seen amateurs who declared that they experienced these sensations when trying without success to "read mind" through the wires, or perhaps without any connection with the subject whatever. Persons who are in the vicinity of galvanic batteries, even though not in the circuit, very often report similar experiences.

The facts which sustain the theory that the so-called mind-reading is really muscle-reading—that is, unconscious muscular tension and relaxation on the part of the subject—may be thus summarized:

1. Mind-readers are only able to find *direction* and *locality*, and, in order to find even these, they must be in physical connection with the subject, who must *move* his body or some portion of it—as the fingers, hand, or arm. If the subject sits perfectly still, and keeps his fingers, hand, and arm, perfectly quiet, so far as it is possible for him to do so by conscious effort, the mind-reader can never find even the *locality* on which the subject's mind is concentrated; he can only find the *direction* where the locality is. Mind-readers never tell what an object is, nor can they describe its color or appearance; *locality*, and nothing more definite than locality, is all they find. The object hidden may be a coin or a corn-cob, a pin or a pen-holder, an elephant's tusk or a diamond-pin—it is all the same. Again, where connection of the operator with the subject is made by a wire, so arranged that mass-motion cannot be communicated, and the subject concentrates his mind ever so steadily, the operator does just what he would do by pure chance, and no more. This I have proved repeatedly with good subjects and expert performers.

2. The subject can successfully deceive the operator in various ways—first of all, by using muscular tension in the wrong direction, and muscular relaxation at the wrong locality, while at the same time the mind is concentrated in the right direction. To deceive a good operator in this way is not always easy, but after some practice the art can be acquired, and it is a perfectly fair test in all experiments of this nature.

Yet another way to deceive the mind-reader is, to think of some object or locality at a great distance from the room in which the experiments are made, and, if there be no ready means of exit, the performer will be entirely baffled. I am aware that some very surprising

feats have been done in the way of finding distant out-of-door localities by muscle-readers, but in these cases there has usually been an implied understanding that the search was to be made out-of-doors; muscle-readers have thus taken their subject up and down stairs or from one room or hall into another, and out-of-doors until the house or locality was reached.[1]

Another way in which deception may by practised is for the subject to select some object or locality on the person of the muscle-reader. This object may be a watch, or a pocket-book, or a pencil-case, or any limited region of his clothing, as a button, a cravat, or wristband. If such a selection be made, and the method of physical connection above described be used, the experiment will be a failure, provided the muscle-reader does not know or suspect that an object on his own person is to be chosen. Similarly, if the subject selects a locality on his own person, as one of the fingers or finger-nails of the hand that connects with the muscle-reader. When such tests are used, there is not, so to speak, any leverage for the tension of the arm toward the locality on which the mind is concentrated, and the muscle-reader either gets no clew, or else one that misleads him.

3. When a subject, who has good control over his mental and muscular movements, keeps the arm connected with the operator *perfectly stiff*, even though his mind be well concentrated on the hidden object, the operator cannot find either the direction or the locality. This is a test which those who have the requisite physical qualifications can sometimes fulfill without difficulty.

Here I may remark that the requirement to concentrate the mind on the locality and direction sought for all the time the search is being made is one that few, if any, can perfectly fulfill. Any number of distracting thoughts will go through the best-trained mind of one who, in company with a blindfolded operator, is being led furiously up and down aisles, halls, streets, and stairways, fearful each moment of stumbling or striking his head, and followed, it may be, by astonished and eager investigators. And yet these mental distractions do not seem to interfere with the success of the experiment unless the arm is kept studiously rigid, in which case nothing is found save by pure chance. The best subjects would appear to be those who have moderate power of mental concentration and slight control over their muscular movements. Credulous, wonder-loving subjects are sometimes partially entranced through the emotions of reverence and expectation; with subjects in this state, operators are quite sure to succeed.

[1] In Danielsonville, Connecticut, Brown, after an evening's exhibition in which his failures had been greater than usual (the intelligent committee having the matter in charge being prepared by previous discussion of the theory of unconscious muscular motion), took a subject, and led him from the hotel in the darkness through the streets, to some rather out-of-the-way building on which the subject had fixed his mind. A somewhat similar exploit is recorded of Corey, a performer in Detroit.

4. The uncertainty and capriciousness of these experiments, even with expert operators, harmonize with the explanation here given. Even with good subjects all mind-readers do not uniformly succeed; there is but little certainty or precision to the average results of experiments, however skillfully performed. An evening's exhibition may be a series of successes or a series of failures according to the character of the subjects; and even in the successful tests the operator usually must try various directions and many localities, sometimes for ten or fifteen minutes, before he finds the locality sought for; cases where the operator goes at once in the right direction, stops at the right locality, and *knows* when he has reached it, are exceptional.[1]

5. Many of those who become expert in this art are aware that they succeed by detecting slight muscular tension and relaxation on the part of the subject.

Some operators have studied the subject scientifically, and are able to analyze with considerable precision the different steps in the process. In the minds of many this fact alone is evidence adequate to settle the question beyond doubt.

6. A theoretical and explanatory argument is derived from the recent discovery of motor centres in the cortex of the brain.

I was repeating the experiments of Fritsch and Hitzig at the time when my attention was first directed to the remarkable exhibitions of Brown, and the results of my studies in the electrical irritation of the brains of dogs and rabbits suggested to me the true explanation of mind-reading before any opportunity had been allowed for satisfactory experiments.

The motto "when we think we move," which I have sometimes used to illustrate the close and constant connection of mind and body, seems to be justified by these experiments on the brain, and may assist those who wish to obtain a condensed statement of the physiology of mind-reading. Taking into full consideration the fact that all physiologists are not in full accord as to the interpretation to be given to these experiments, whether, for example, the phenomena are due to direct or reflex action, still it must be allowed, by all who study this subject experimentally, that thought-centres and muscle-centres are near neighbors, if not identical.[2]

[1] The popular theory to account for these failures is the weariness or exhaustion of the operator; but both in New York and in New Haven it was observed that Brown met with his most brilliant successes in the latter part of the evening, the reason being that he happened then to have better subjects.

[2] From an editorial in the *Boston Medical and Surgical Journal* (September 23, 1875), referring to the mind-reading exhibitions, and accepting the explanation here given, I make the following extract:

"The whole performance seems to us to furnish good illustrations of one or two well-known principles of great physiological interest. Of these the most important is one that finds at once support and application in the modern doctrine of the nature of aphasia

In all these experiments it should be observed there is no one muscle, there is no single group of muscles, through which this tension and relaxation are developed; it is the finger, the hand, the arm, or the whole body, according to the method employed. Among the various methods of making connection between the subject and operator are the following:

1. The back of the subject's hand is held firmly against the forehead of the operator, who, with his other hand, lightly touches the fingers of the subject's hand. (Brown.)

This is, undoubtedly, the most artistic of all known methods.

2. The hand of the operator loosely grasps the wrist of the subject.

This is a very inartistic method, and yet great success is oftentimes attained by it.

3. One finger of the operator is applied to one finger of the subject, papillæ touching papillæ.

This is a modification of the first method; by it exceedingly small objects or localities are found.

4. The operator is connected in the usual way with a third party who does not know the locality thought of by the subject, but is connected with the subject by the wrist ("double test").

In this experiment, which astounded even the best observers, the unconscious muscular motion was communicated from the subject to the arm of the third party, and through the arm of the third party to the operator.

5. Two, three, or more subjects, who agree on the locality to be thought of, apply their hands to the body of the operator in front and behind.

This method is excellent for beginners, and the direction is easily found by it; but it is obviously not adapted for the speedy finding of small objects; it is frequently used by ladies.

6. The hand of the subject lightly rests on the shoulder of the operator.

and kindred disorders; namely, that the thought, the conscious mental conception, of an act, differs from the voluntary impulse necessary to the performance of that act only in that it corresponds to a fainter excitation of nervous centres in the cortex cerebri, which in both cases are anatomically identical.

"Thus, in certain forms of aphasia the power to think in words is lost at the same time with the power of speech. Some persons think definitely only when they think aloud, and it would readily be believed in the case of children and uneducated persons that the ability to read would often be seriously interfered with if they were not permitted to read aloud. Similarly, a half-premeditated act of any kind slips often into performance before its author is aware of the fact. Further, there is reason to think, from the experiments of Hitzig, that these same centres may be excited by the stimulus of electricity so as to call out some of the simpler coördinated movements of the muscles on the opposite side of the body.

"Applying, now, this principle to the case in hand, it will be evident that for the person experimented with to avoid giving 'muscular hints,' of either a positive or a negative kind, would be nearly impossible."

In all these methods the operator is usually blindfolded, so that he may get no assistance from any other source than the unconscious muscular action of the subject.

The movements of the operator in these experiments may be either very slow, cautious, and deliberate, or rapid and reckless. Brown, in his public exhibitions, was very careful about getting the physical connection right, and then moved off very rapidly, sometimes in the right direction, sometimes in the wrong one, but frequently with such speed as to inconvenience the subject on whom he was operating. These rapid movements give greater brilliancy to public experiments and serve to entrance audiences, but they are not essential to success. They serve, no doubt, in many cases, to bewilder or partially entrance the subject, and thus to render him far more likely to be unconscious of his own muscular tension and relaxation through which the operator is guided.

The power of muscle-reading depends mainly, if not entirely, on some phase of the sense of touch. Dr. Hanbury Smith tells me that a certain maker of lancets in London had acquired great reputation for the superiority of his workmanship. Suddenly, there was a falling off in the character of the instrument that he sent out, and it was found that his wife, on whom he had depended to test the sharpness of the edge on her finger or thumb, had recently died.

That the blind acquire great delicacy of touch has long been known; Laura Bridgman is a familiar illustration. Dr. Carpenter states (although there are always elements of error through the unconscious assistance of other senses in cases of this kind) that Miss Bridgman recognized his brother, whom she had not met for a year, by the touch of the hand alone.

Every physician recognizes the fact of this difference of susceptibility to touch; and, in the diagnosis of certain conditions of disease, much depends on the *tactus eruditus*. I am not sure whether this delicacy of perception, by which muscle-reading is accomplished, is the ordinary sense of touch, that of contact, or of some of the special modifications of this sense. It is to physiologists and students of diseases of the nervous system a well-known fact that there are several varieties of sensibility—to touch, to temperature, to pressure or weight, and to pain—which, possibly, represent different rates or modes of vibration of the nerve-force.

The proportion of persons who can succeed in muscle-reading, by the methods here described, is likewise a natural subject of inquiry. Judging from the fact that, out of the comparatively few who have made any efforts in this direction, a large number have succeeded after very little practice, and some few, who have given the matter close attention, have acquired great proficiency, it is probable that the majority of people of either sex, between the ages of fifteen and fifty, could attain, if they chose to labor for it, under suitable

instruction, a certain grade of skill as muscle-readers, provided, of course, good subjects were experimented with. It is estimated that about one in five or ten persons can be put into the mesmeric trance by the ordinary processes; and, under extraordinary circumstances, while under great excitement, and by different causes, every one is liable to be thrown into certain stages or forms of trance; the capacity for the trance-state is not exceptional; it is not the peculiar property of a few individuals—it belongs to the human race; similarly with the capacity for muscle-reading. The age at which this delicacy of touch is most marked is an inquiry of interest; experience, up to date, would show that the very young or the very old are not good muscle-readers. I have never known of one under fifteen years of age to study this subject; although it is conceivable that bright children, younger than that age, might have sufficient power of attention to acquire the art, certainly if they had good instruction in it.

In these mind-reading experiments, as indeed in all similar or allied experiments with living human beings, there are six sources of error, all of which must be absolutely guarded against if the results are to have any precise and authoritative value in science.

1. The involuntary and unconscious action of brain and muscle, including trance, in which the subject becomes a pure automaton. I have used the phrase "involuntary life" to cover all these phenomena of the system that appear independently of the will. The majority of those who studied the subject of mind-reading—even physicians and physiologists—failed through want of a proper understanding or appreciation of this side of physiology.

2. Chance and coincidences. Neglect of this source of error was the main cause of the unfortunate results of the wire and chain experiments with mind-readers.

3. Intentional deception on the part of the subject.

4. Unintentional deception on the part of the subject.

5. Collusion of confederates. To guard against all the above sources of error it is necessary for the experimenter himself to use deception.

6. Unintentional assistance of audience or by-standers.

When the muscle-reader performs before an enthusiastic audience, he is likely to be loudly applauded after each success; and, if the excitement be great, the applause, with shuffling and rustling, may begin before he reaches the right locality, while he is approaching it; when, on the other hand, he is far away from the locality, the audience will inform him by ominous silence. The performance thus becomes like the hide-and-seek games of children, where they cry "Warm!" as the blindfolded operator approaches the hidden object; "Hot!" as he comes close to it; and "Cold!" when he wanders far from it. Some of the apparent successes with the wire-test may be thus explained.

In regard to all the public exhibitions of muscle-readers, it should

be considered that the excitement and *eclat* of the occasion contribute not a little to the success of the operator; the subjects grow enthusiastic—are partly entranced, it may be—become partners in the cause of the performer—and unconsciously aid him far more than they would do in a similar entertainment that was purely private. In a private entertainment of muscle-reading at which I was present, one of the subjects, while standing still, with his hands on the operator, actually took a step forward toward the locality on which his mind was concentrated, thus illustrating in a visible manner the process by which muscle-reading is made possible.

The subject under discussion, it will be observed, is to be studied both inductively and deductively. The general claim of mind or thought reading is disproved not by any such experiments as are here detailed, no matter how accurate or numerous they may be, but by reasoning deductively from the broad principle of physiology, that no human being has or can have any qualities different in *kind* from those that belong to the race in general. The advantage which one human being has over another—not excepting the greatest geniuses and the greatest monsters—is, and must be, of degree only. Mind-reading, in the usual meaning of the term, is a faculty that in any degree does not belong—indeed, it is never claimed that it belongs—to the human race; it cannot, therefore, belong to any individual. For one person to read the thoughts of another would be as much a violation or apparent violation of the laws of Nature as the demonstration of perpetual motion, the turning of iron into gold, or the rising of the sun in the west. Experiments such as here recorded, if made for the purpose of ascertaining whether certain persons have the power of reading thoughts, would be more than unnecessary; they would be exceedingly unscientific. Reasoning deductively also from the known laws of the involuntary life, the power to read muscles, in the method here described, is not only possible and probable, but inevitable. Everybody is a muscle-reader, although all are not capable of attaining the highest degrees of skill in the art.[1]

The one fact, the only fact brought out by these experiments that could not be predicted from known laws of physiology, is the exceeding refinement to which muscle-reading can be carried, the minuteness of the localities that are found, and the rapidity with which, oftentimes, the results are obtained. This fact is of permanent value to science, a new and positive addition to the physiology of the involuntary life, and of vast suggestion in relation to the general subject of the interactions of mind and body in health and in disease.

An incidental fact impressed on my mind during these researches

[1] Every horse that is good for anything is a muscle-reader; he reads the mind of his driver through the pressure on the bit, and by detecting tension and relaxation knows when to go ahead, when to stop, and when and which way to turn, though not a word of command is uttered.

was the prevalence and the power of the belief in animal magnetism. This delusion may well be regarded as the witchcraft of the nineteenth century; its hand is everywhere—on the press and the pulpit, on all our literature, on science itself, even on physiology, to which its phenomena rightly belong, and by which they can be and are fully explained. It is a tyrant that rules over the whole realm of the seemingly mysterious; the success of the orator on the platform, and of the physician at the bedside, is attributed to its aid, as of old superior learning and skill were attributed to the occult forces of magic. It may be doubted whether any other false belief of our time has had a more serious influence in retarding the progress of right reasoning than this, since it blocks the doors of investigation and prejudges the case when investigations are made, stimulates the too common habit of making the emotions do the work of the intellect, and becomes a sort of foster-mother to other and allied delusions.

It was the universality of this belief in animal magnetism that made mind-reading popular, since it furnished a basis as broad as the wildest theorizer could wish, on which could be erected a limitless variety of hypotheses; and many who rejected intuitively the claim of direct supernatural aid were made happy by the equally false and untenable claim of literal conveyance of thought from subject to operator through the agency of a supposed magnetic fluid.

## COMPRESSED-AIR LOCOMOTIVE IN ST. GOTHARD TUNNEL.[1]

By C. M. GARIEL.

THE boring of a tunnel of any importance presents difficulties of various kinds, among which may be mentioned the clearing away of the rubbish arising from the excavation of the gallery, whenever that reaches any considerable length, and the work is carried on with activity. Such were the conditions under which the boring of the Mont Cenis Tunnel was carried on, and M. Fabre, the able contractor, has met with similar difficulties in the boring of the St. Gothard Tunnel, now being carried out.

The work was begun from two points, Airolo and Gœschenen, the two extremities of the future tunnel. The advance of the gallery, which is pushed on with activity, produces about 400 cubic metres of rubbish a day at each of the two faces of attack. To carry away this mass of rubbish, which is thrown regularly into trucks running on rails, it is impossible to employ locomotives, as the *cul-de-sac* nature

[1] Translated from *La Nature*.

of the galleries prevents effectual ventilation. The high price of horses and the large number required prevent their use. The idea suggested itself of making use for St. Gothard of machines moved by compressed air, which would have many advantages. First, it is well known that compressed air is used to work the perforating machines used in boring the tunnel; then, by the employment of compressed-air locomotives, ventilation of the galleries would be produced, as these machines would allow only pure air to escape; and then these motors would be more powerful than horses, and effect more rapidly the clearing away of the *débris*.

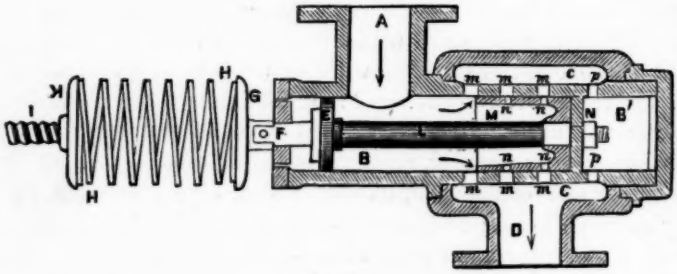


FIG. 1.

A first attempt was made in which two ordinary locomotives were employed, one at each side of the tunnel; the boilers, in which, of course, there was no water, were filled with condensed air under a pressure of four atmospheres. This air played the part usually done by steam, passed into slide-valves, entered the cylinders alternately on each face of the pistons, which it set in motion, and then escaped into the atmosphere.

It is easily seen that, if compressed air were to be employed, it would be indispensable to have a very considerable quantity of it; the boiler of a locomotive, sufficient when it is worked by means of steam constantly produced under the action of heat, was too small to contain a quantity of air sufficient for use without being filled. This led to adding to each locomotive a special reservoir for compressed air; each locomotive was accompanied, as a kind of tender, by a long sheet-iron cylinder, eight metres long and one and a half metre diameter, supported toward its extremities by two trucks, which, on starting, were filled with condensed air, and which communicated by a tube with the distributing apparatus of the cylinders. The locomotive then worked as before, except that compressed air came from the reservoirs instead of from the boiler. The two locomotives, the Reuss and the Tessin, worked economically for about two years, in spite of the awkwardness of the long cylinders that accompanied them. We can give some interesting figures resulting from the mean of a certain number of observations. At departure the pressure in

the reservoir was about seven kilogrammes per square centimetre; the locomotive having drawn a train of twelve loaded wagons along a course of about 600 metres, the pressure was found to fall to four and a half kilogrammes; the train then returned empty to the point of departure, and the final pressure was found to be two and a half kilogrammes.

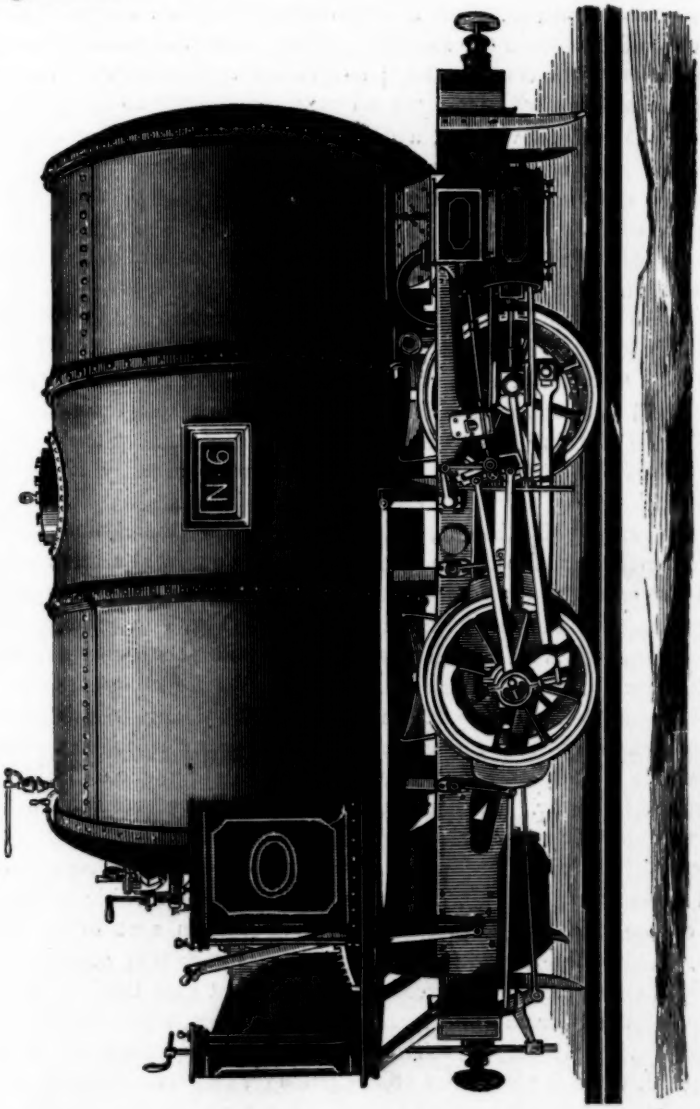


FIG. 2.—COMPRESSED-AIR LOCOMOTIVE USED AT THE ST. GOTHARD TUNNEL WORKS.

In spite of the relatively advantageous results which were obtained, the employment of compressed air in a steam-locomotive presented a certain number of drawbacks. It is expedient that the air

should issue from the cylinder under the least possible pressure, in order that refrigeration may be reduced to a minimum; for it is known that the expansion of gas is accompanied by a loss of heat which increases with the pressure. This condition was satisfied by causing the air to act under restraint; that is, by allowing the compressed air coming from the reservoir to enter during only a part of the course of the piston. But the admission of the air ought to vary if it is desired to obtain the same final effect, since the pressure in the reservoir diminishes continuously; and as the apparatus which regulates the admission was arranged to correspond only to determined fractions, but not to vary in a continuous manner, it followed that there was a greater expenditure of air than was necessary, and consequently a diminution in the length of the course over which the locomotive could run.

On the other hand, it is necessary that the air should arrive in the distributing apparatus with the least possible pressure, for it is in this apparatus, in the slide-valve, that the greatest losses take place, and these losses increase in proportion to the pressure. No means could, however, be thought of for diminishing the pressure in the reservoirs, which would have reduced considerably the work which the machines were capable of doing, unless by augmenting considerably the volume of the reservoirs, the dimensions of which were already unusually large.

At this stage M. Ribourt, the engineer of the tunnel, devised an arrangement which allows the compressed gas to flow at a fixed pressure, whatever may be the pressure in the reservoir. The gas in escaping from the reservoir enters a cylinder *B* (Fig. 1), over a certain extent of the walls of which are openings *m m*, that communicate with another cylinder *C*, which surrounds it to the same extent, and which is connected with the slide-valve by which the air is distributed, or, more generally, with the space in which this air is to be utilized. On one side moves a piston *E*, which shuts the cylinder and hinders the escape of the air. This piston carries externally a shaft *F*, which supports externally a spiral spring *H*, the force of which is regulated by means of a screw. Internally it is connected by another shaft *L* with a second piston *N*, which bears a cylinder *M*, movable in the interior of the principal pump, and forming thus a sort of internal sheath. This sheath presents openings *n n*, which may coincide exactly with those already referred to, and in that case the gas passes without difficulty from the reservoir at the point where it is to be employed. But if the sheath is displaced, the openings no longer correspond, there is resistance to the passage, and consequently diminution of the quantity of gas which flows out, and hence lowering of pressure in the exterior cylinder. By making the position of the sheath to vary continuously we may make the pressure of exit constant, notwithstanding the continuous variation at entry. But the

apparatus is automatic. In fact the part of the cylinder *D* comprised between the bottom and the piston *N* communicates by openings *p* (which are never covered with the escape-tube of the gas), in such a manner that upon its posterior face the piston *N* receives the pressure of the gas at the moment when it flows, a pressure which it is sought to render constant. The piston *E* receives on its anterior face the action of the spring which can be regulated at pleasure. As to the other faces of the two pistons, they are subjected to equal actions proceeding from the pressure of the gas at its entry, actions which thus counteract each other; so that the forces which determine the position of the movable system are, on the one hand, the tension of the spring, a constant and determined force, and, on the other hand, the pressure of the flowing gas; and thus equilibrium cannot occur unless the two forces are equal. If the gas should flow in too great a quantity, the pressure increases on the posterior face of the piston *N*, the spring is overcome, and the movable system advances a little toward the left; but then the orifices are partly covered and the flow diminishes. If the pressure then becomes too weak at the exit, the spring in its turn prevails, pushes the sheath toward the right, uncovers the orifices, and consequently a greater quantity of air may enter.

The machines which are now used at the St. Gothard Tunnel, genuine compressed-air locomotives, are furnished with M. Ribourt's apparatus. They consist of the following parts: A sheet-iron reservoir to contain the compressed air is mounted on a framework quite like that of steam-locomotives, and carrying glasses, cylinders, distributing apparatus, etc. The tube for receiving the air possesses, within reach of the driver, the automatic valve of M. Ribourt. The screw being easily regulated, the air can with certainty be made to issue from the apparatus at a determined pressure. This air then passes into a small reservoir (about one-third metre cube), intended to deaden the shocks, which are always produced when the machine is set agoing or stopped. Lastly, this small reservoir communicates with the cylinders, and the air which reaches them acts in the same manner as steam in ordinary locomotives.

The pressure in the principal reservoir at the point of exit depends on the power of the compressing apparatus; at St. Gothard it may attain fourteen kilogrammes per square centimetre, but is ordinarily about 7.35 kilogrammes. The pressure in the small reservoir is arbitrary, depending on the regulation of the screw; at St. Gothard it has a mean of 4.20 kilogrammes. The entire machine weighs about seven tons.—*Nature.*

# GAS MANUFACTURE AND GAS COMPANIES.[1]

CONDENSED FROM THE BOSTON REPORT,

By WILLIAM E. SIMMONS.

THERE are three kinds of gas, named after the substances from which they are obtained, as coal, petroleum (or naphtha), and water gas. The first two are produced by destructive distillation of coal and petroleum, or naphtha, the last by passing a current of steam over a bed of incandescent anthracite. Coal-gas is the kind in general use, petroleum or naphtha gas being used chiefly as an enricher; and water-gas, used in only a few places and for a comparatively short time, is as yet on its trial. The general principles involved in the manufacture of each kind of gas will first be noticed; then the relative merits of the products will be considered in regard to quality, cost of manufacture, and respective peculiarities; and, finally, a comparative view will be taken of several leading companies; after which the relationship of gas companies to municipalities, and the subject of competition, will be examined.

When coal is subjected to high heat in a closed vessel, certain gases and vapors are evolved, some of which are combustible, and some, like steam, condensible, a residue of charcoal or coke being left behind. This process is termed destructive distillation, and the property displayed by coal is common to all vegetable and animal substances; but only coal and petroleum have been used economically in the production of illuminating gas on an extensive scale. The distillation is the most important operation in the manufacture, but it is necessary to remove from the gas, before it is fit for burning, the condensible vapors, as tar, water, etc.; and those non-condensible gases, as carbonic anhydride (carbonic acid), which either largely diminish the illuminating power, or which give rise, in the burning of the gas, to injurious products of combustion, such as sulphuretted hydrogen and ammonia. The removing of these very materially affects the cost of production. The distillation is effected in iron or clay

[1] Abstract of a Report to the City of Boston, A. D. 1876, by the Gas Commissioners, Messrs. Charles F. Choate, John Felt Osgood, and Edward S. Wood, appointed in January, 1875, who were instructed "to investigate and report—1. On the quality and price of the gas furnished in Boston, as compared with that of other large cities in this country and Europe; 2. Whether any improvements can be made in the present methods of manufacturing gas by the different companies in this city; 3. Whether it would be expedient for the city to undertake the manufacture and supplying of gas for public and private lighting; and, 4. Whether any further legislation is desirable to enable gas-consumers, or the municipal authorities, to secure a prompt and impartial investigation of complaints against private companies, and an efficient remedy for any abuses of which they may be found guilty."

retorts, from three to seven of which, according to circumstances, are heated with one fire of coke to a cherry-red (1,478° to 1,830° Fahr.) for iron, or to an orange or white heat (2,000° to 2,300° Fahr.) for clay retorts; 160 to 260 pounds of coal constitute the charge for a single retort, and the distillation continues uninterruptedly for four to four and a half hours. The outer layers of the charge, being suddenly raised to a high temperature, evolve vapors which contain a large amount of carbon. These, in passing through the retort, are converted into fixed gases of a high illuminating power. The inner parts of the charge, undergoing distillation more slowly, give out vapors which, in passing through the highly-heated coke on the surface, are more completely decomposed than the first evolved, and are, therefore, of a lower illuminating power. It has been shown, for example, by Mr. C. D. Lamson, of the Boston Gas-Works, that the illuminating (or candle) power of the gas diminishes in a rapidly-increasing ratio with each half-hour of the distillation; and also that, after the third half-hour, the quantity of gas produced similarly decreases. The largest quantity, as well as the richest gas, is, therefore, obtained in the first part of the distillation. By *candle-power* is meant that the gas, burning at the rate of five cubic feet per hour, will give as much light as the stated number of standard sperm-candles burning at the rate of 120 grains per hour, or two grains per minute.

The gas passes next into the hydraulic main, where it is made to bubble up through a half-inch to an inch of water, and thus some of its vapors are condensed. It then goes to the condenser, a series of iron tubes surrounded by water, to be cooled and more completely rid of its tar and other vapors, which are precipitated and led away. Going to the washers, a series of chambers where it is brought in contact with jets of water, and to the scrubbers, where it passes through a collection of coke, fire-brick, etc., moistened with water, it is relieved of the rest of the tar and also of the ammonia.

The third step in the manufacture is purification, which removes from the gas the noxious elements, chiefly carbonic acid and sulphuretted hydrogen. The first lowers the illuminating power very greatly: one per cent. being sufficient, it is said, to diminish it five per cent. The sulphuretted hydrogen and other sulphurous compounds give rise in burning to sulphurous and sulphuric acids which may injure, by their corrosive action, delicate structures, such as books, gilding, silks, etc., that are exposed to the air of the room in which the gas is burned. Lime and oxide of iron are used in various methods to purify the gas. Lime is used both wet and dry. In the wet-lime process the gas is passed through the milk of lime, which, uniting with the carbonic acid to form a chalk, effectually removes it, and takes away most of the sulphur compounds too, by uniting with them to form calcic sulphide or calcic sulpho-carbonate.

The use of this process has generally been abandoned, however,

on account of the foul odor evolved from the lime when it is taken from the purifier. The dry-lime process consists in passing the gas through moistened slacked lime placed upon trays. This is about as effective as the other, and has generally superseded it. The iron process consists in passing the gas through some form of the hydrated sesquioxide of iron mixed with other substances. The great advantage of this process is its economy, it being practicable to use the same mixtures over and over almost indefinitely. The New York Mutual Company, for instance, have used one mixture satisfactorily for three years.

Either petroleum or some of the products of its distillation at a low temperature, as naphtha, rhigolene, gasolene, etc., may be used in the manufacture of gas. These products are of little commercial value as compared with those, like kerosene, which are produced at a higher temperature, but for this reason they are of especial value for the manufacture of gas. The principles on which the manufacture of petroleum-gas depends do not differ much from those involved in the making of coal-gas. In both cases, as already stated, the material is subjected to destructive distillation in a retort; but in this the material may either be introduced directly into the retort, or first converted into a vapor, and conducted into it in that state. The first step, however, is to vaporize the liquid either in the retort or before it reaches there; and the second, to decompose the vapor, and convert it into a fixed gas, which is carried into an hydraulic main and condenser in the same way as coal-gas. One great advantage of the naphtha-gas is that, containing neither sulphur compounds nor ammonia, it needs no purification, and therefore saves one item in the expense of manufacture. Moreover, a loss of some of the luminiferous hydrocarbons is avoided, a certain amount of them being necessarily condensed in the passage through the washers, scrubbers, and purifiers.

The manufacture of water-gas differs entirely from that of coal or naphtha gas. It involves the production, first, of a non-illuminating gas from steam, and, second, of petroleum, naphtha, or cannel gas, to furnish the illuminating power. The great advantage of it is, that very large volumes of non-luminous combustible gas can be made very cheaply. This is done by passing steam over incandescent carbon, which, having a very powerful attraction for oxygen, abstracts it from the steam (water being a compound of hydrogen and oxygen), and unites with it, forming, at first, carbonic acid. This, in passing over another bed of coal, is deprived in turn of one-half its oxygen, and converted into carbonic oxide. Hydrogen, the other constituent of the steam, being set free, mixes with the carbonic oxide. The resultant is a mixture of hydrogen and carbonic oxide, which gases are both combustible but non-illuminating. In some processes for the manufacture of this gas, the petroleum or naphtha gas

is not mixed with the water-gas until the latter has been purified; in others the petroleum is added directly to the coal. Anthracite coal, only, is used in the manufacture of water-gas, and great care is necessary to keep the temperature up to a white heat, since, if it falls too low, a large proportion of carbonic acid will be formed, and will injure the illuminating power of the gas unless it is removed by purification. Anthracite coal contains sulphur, and yields ammonia when distilled, so that purification is as necessary for water as for coal-gas, and therefore no saving is made in this respect. The real saving is in coal, since a large volume of steam can be decomposed by one ton.

It is necessary now to make a more particular inquiry into the nature of the different gases and compare them with each other. Coal-gas in its salable condition is composed about as follows:

| NAMES. | Heidelberg. | Bonn. | Chemnitz. | London, Common. | London, Cannel. |
|---|---|---|---|---|---|
| Hydrogen | 44.00 | 39.80 | 51.29 | 46.00 | 27.70 |
| Marsh-gas | 38.00 | 43.12 | 36.45 | 39.50 | 50.00 |
| Carbonic oxide | 5.73 | 4.66 | 4.45 | 7.50 | 6.80 |
| Olefiant and other hydrocarbons | 7.27 | 4.75 | 4.91 | 3.80 | 13.00 |
| Nitrogen | 4.23 | 4.65 | 1.41 | 0.50 | 0.40 |
| Oxygen | .... | .... | 0.41 | .... | .... |
| Carbonic anhydride | 0.37 | 3.02 | 1.08 | 0.70 | 0.10 |
| Steam | .... | .... | .... | 2.00 | 2.00 |

The hydrogen and carbonic oxide burn with a non-luminous flame, and marsh-gas burns with only a slightly-luminous one, the illuminating power coming almost entirely from the olefiant gas and other hydrocarbons. The oxygen, carbonic acid, and nitrogen, being incombustible, injure the illuminating power very greatly. The oxygen and nitrogen are admitted accidentally by the introduction of a little air in charging the retorts. Hence, other things being equal, the illuminating power of the gas increases with the proportion of olefiant and other hydrocarbons, and these depend chiefly on the kind of coal used and the temperature at which it is carbonized. The gas-coals are the bituminous caking coals and cannel. The bituminous shales, like the boghead mineral of Scotland, and asphalt minerals, like the Albertite of Nova Scotia and the Grahamite of West Virginia, are used in small quantities for enriching. The yield of good gas-coal, like the Penn, is about 10,000 cubic feet of 15 to 16 candle-power gas to the ton (2,240 lbs.). The enriching coals yield a larger amount of richer gas, and the asphalt minerals from 13,000 to 15,000 cubic feet of 30 to 50 candle-power. The relative cost of enriching with these or with naphtha is a very important question. In a number of experiments made at the Boston Gaslight Works, for the purpose of testing the value of Albertite as an enricher, it was found that the

yield varied from 13,440 to 16,016 cubic feet of 36.08 to 54.45 candle-power gas per ton (6 to 7.15 feet per pound). Experiments on cannel of different kinds have given various yields, from 3.8 feet of 20.52 candle-power gas per pound to 4.74 feet of 30.40 candle-power.

The principal impurities in coal-gas are, as already indicated, sulphuretted hydrogen and other gases containing sulphur and ammonia. The presence of a small amount of these cannot be avoided, and is not injurious, since the sum of the products of the combustion of these substances, formed by the burning of the gas from a single burner during an entire evening, is very small. In fact, the presence of a slight amount of ammonia is beneficial, in tending to neutralize the sulphurous and sulphuric acids formed by that portion of the sulphur which cannot be renewed. The limit prescribed by law for the London companies is 20 grains of sulphur and 2½ grains of ammonia per 100 cubic feet. The specific gravity of illuminating gas is an important quality, since it increases in a nearly constant ratio with the candle-power, so that, knowing the one, the other can be told pretty nearly, and *vice versa*. It varies usually from 0.400 to 0.500; the specific gravity of air, 1.000, being taken for the unit. The denser the gas the more slowly it will pass through a given orifice, and so on this quality depends in great measure the quantity which passes through the consumer's meter and burner.

In regard to petroleum as a gas-making material experiments made by Prof. A. Wagner show that naphtha is better and more economical than it or any of the heavy oils. Fifty kilogrammes of petroleum produced 1,547 cubic feet of gas, while the same amount of naphtha produced 1,619 cubic feet. Both petroleum and naphtha produce a large amount of acetylene, a gas which contains a large proportion of carbon. In the experiments referred to, five per cent. of acetylene was evolved, 35.96 per cent. of other heavy (rich) hydrocarbons, and 59 per cent. of light (poor) hydrocarbon gas; the petroleum being split up, with deposition of carbon, into a mixture of acetylene, heavy and light hydrocarbon gas, and hydrogen. In this country, where petroleum and its products are much cheaper than they are in Europe, it has been found that on a large scale 60 to 80 cubic feet of 50 to 70 candle-power gas can be made from one gallon. In a series of thirteen experiments on crude petroleum made by Mr. C. D. Lamson, of the Boston Gaslight Company, the average yield per gallon was 72.71 cubic feet, and the average of six tests of candle-power was 45.73. In seven experiments on naphtha the average yield was 79 cubic feet, candle-power 53.48. This is equivalent to a yield per barrel respectively of 3,053.82 and 3,338.58 cubic feet. According to Mr. J. D. Patton, about 70 cubic feet of 80 candle gas, or 80 feet of 70 candle gas to the gallon, is the maximum yield of petroleum or naphtha. A much smaller burner than the ordinary must be used for gas obtained from pure Albertite, petroleum, or naphtha; otherwise the flame will smoke,

and much light be lost, although, on account of their greater specific gravity, less of any of such gases would pass through the same burner in a given time.

Reference has been made to the large quantity of water-gas that is made by one ton of coal. In one of the processes employed, the average result of four months' working was 1,000 cubic feet of gas to 67.71 pounds of coal and 3.22 gallons of crude petroleum, or 33,082 cubic feet of about 19 candle gas to one ton of coal and 106.5 gallons petroleum: allowing 70 cubic feet for each gallon of petroleum, or 7,335 feet for the whole, there remains 25,745 feet for the one ton of coal. It will be remembered that the amount of gas obtained by distillation, from the very best coal, ranged between 10,000 and 16,000 cubic feet. Water-gas contains as a rule 40 to 50 per cent. of hydrogen, 30 to 40 per cent. of carbonic oxide, 10 per cent. of naphtha or petroleum gas, and a few per cent. of carbonic acid. The large proportion of heavy petroleum-gas (sp. gr. 0.600 to 0.700) and carbonic oxide (sp. gr. 0.967) makes its specific gravity much heavier than that of coal-gas; but the hydrogen, which is the lightest known gas (sp. gr. 0.067), brings it down to between 0.500 and 0.600.

It has been seen that the gas of common coal is of comparatively poor illuminating power, unless enriched by the gas of other coals or of petroleum, and that water-gas of itself possesses no illuminating power whatever. Although, in considering the nature of the different gases, their relative values were incidentally compared, it is necessary to speak of them now more particularly. In regard to quality, it has been shown that naphtha-gas is the purest, since it contains no sulphur or ammonia, and that it is the richest, being from 60 to 80 candle-power, while common coal-gas is only from 15 to 20 candle-power. It is also the most economical, alike for producer and consumer: for the consumer, because, owing to its higher specific gravity, it burns much more slowly than the coal-gas, while it also gives a better light. The higher the specific gravity of the gas, the longer it will take to pass through a given orifice, and therefore the more slowly it will consume; and the higher the candle-power, the less gas is burned in giving the same amount of light. It is more economical for the producer, because, in the first place, there is a great saving in its manufacture, in the handling of the material. The retorts can be supplied continuously, and the frequent interruptions for recharging necessary, in the use of coal, are avoided. Each retort, too, can be made to produce a much larger amount of gas in the one case than in the other. About 10,000 feet of petroleum-gas can be made daily with a single retort, against about 5,000 feet of coal-gas; and 60,000 to 70,000 cubic feet per day, per stoker, against 25,000 to 30,000. Hence there is a saving both of labor and wear and tear of works. The cost of works for making pure petroleum-gas is also much less than that of coal-gas works. Notwithstanding these facts, the commissioners, whose report we have

been considering, did not deem it practicable to change at once to the use of pure petroleum or naphtha gas in Boston, as the burners in use are, for reasons already given, not suitable; the works employed to produce coal-gas are not adapted to this, and as the flame of petroleum-gas "burning in an appropriate burner is a very small flame," it would not in their opinion prove satisfactory to consumers, although the amount of light would be the same if not greater. The objection that petroleum-gas in any form injures the metres was found to be without warrant.

The practice which obtains with the Detroit Mutual Company and others, of adding air to naphtha-gas to reduce its illuminating power so that it can be burned in an ordinary burner, was judged by the commissioners to be the reverse of economical, to both the company and consumer, because the deterioration of the gas by this means is in greater ratio than the increase of its volume. It is said that one per cent. of air will reduce the illuminating power six per cent., or more than carbonic acid, the removal of which is considered necessary by all gas-engineers for the sake of economy. It was for this reason that the first attempt to make illuminating gas from petroleum (that at Saratoga by the Gale and Rand process) failed.

What is true of the value of naphtha as a gas-making material used alone, is also true of its value as an enricher. Experiments already here referred to, although not expressed in terms of equality, imply the superiority of naphtha to Albertite, which is about the best of the enriching coals. The yield from a ton of the latter, which costs about $25, was, on the average of a number of experiments made by the Boston Gaslight Company, only 14,694.4 cubic feet of 55 candle gas; while the yield of $25 worth of naphtha (valuing it at ten cents a gallon, which is rather high) would be 19,872.5 cubic feet of 64.5 candle gas, or 5,178.1 cubic feet more gas of richer quality than a ton of Albertite. By the use of naphtha, too, a larger amount of gas is obtained from the ordinary caking coal. In enriching with Albertite the coal with which it is mixed is distilled in an iron retort at a comparatively low temperature; while, if naphtha be used, all of the common coal can be carbonized in a clay retort, which is acknowledged by all to be more economical, and all of the gas in the coal can be exhausted, so that about 1,000 cubic feet more can be obtained per ton. The iron retorts are more expensive than the clay, because their first cost is greater, and they do not last as long. In making gas on a large scale, about one-half the number of retorts can be dispensed with, in the use of naphtha as an enricher. The New York Mutual Gas Company, for example, in this way, make as much gas with forty retorts as can be made in the other with eighty; and with the disuse of the extra forty retorts the labor necessary to tend them is dispensed with. The increased yield of the coal by the use of naphtha, referred to just now, is demonstrated by practical experience to be

very considerable. Comparison of the result of a year's work obtained by the New York Mutual Company, which uses naphtha for an enricher, and the Boston Gaslight Company, which uses Albertite, shows that the former obtained 10,975 cubic feet of 19 to 20 candle-power gas per ton of coal used, while the latter obtained only 8,779 cubic feet of 18 to 19 candle gas, a difference of 2,196 cubic feet per ton in favor of the naphtha.

In regard to the alleged danger in keeping large quantities of naphtha in store, the commissioners say: "There is no doubt that it is more difficult to extinguish burning naphtha than burning coal; but the statements that naphtha is like gunpowder (explosive), and dangerous to store, are erroneous. In fact, it is almost impossible to mix naphtha-vapor and air so as to make an explosive mixture, for the reason that, when the proper amount of oxygen is present, the mixture is diluted with so large a bulk of inert nitrogen that it cannot be ignited." In regard to the supply, they see no reason to fear that it will be inadequate to "any demand which may exist in the future." The oil-region extends over a wide expanse of country, embracing large districts in Pennsylvania, Eastern Ohio, West Virginia, Kentucky, Indiana, and Western Canada. The production in 1874 was 10,910,303 barrels—larger than ever before by more than 1,000,000 barrels; the average price was 2.8 cents per gallon, or $1.17 per barrel.

The objections urged against water-gas are, that its specific gravity is too high; that it contains a large proportion of the extremely poisonous gas, carbonic oxide; and that the manufacture, being in its infancy, is not yet proved to be a success. The first is of no importance, since the specific gravity, unless it is caused by the presence of a large amount of carbonic anhydride, is high in almost exact proportion as the illumination power is great.

The commissioners say of the second objection that it is, in their opinion, "sufficient to entirely prevent the use of the mixed hydrogen and carbonic oxide" (unenriched water-gas) "alone for heating purposes, for the reason that, since it is devoid of odor, its escape from pipes and diffusion through the air of an inhabited room, in dangerous amount, could not be detected. The addition to it of petroleum-gas as an enricher, for illuminating purposes, at once imparts to it a peculiar odor, as strong as that of coal-gas, which would lead to the immediate detection of a leak." Carbonic oxide is one of the most active poisons, producing, when inhaled, speedy death. Unlike carbonic acid, which, when it poisons, does so by merely preventing the entrance of air or oxygen into the lungs, as water does in case of drowning, so that persons affected can be readily resuscitated, it is a true physiological poison. And while the first can be rendered harmless by a moderate dilution with atmospheric air, the last produces death almost as readily when diluted as when pure. It forms a com-

pound with the red coloring-matter of the blood, which is more stable than that formed by carbonic anhydride, and cannot be readily decomposed by oxygen. According to Leblanc, one volume of it diffused through one hundred volumes of air totally unfits it to sustain life; and it appears that the lamentable accidents which too frequently occur from burning charcoal or coke, in braziers and chafing-dishes, in close rooms, result from the poisonous effects of the small amount of carbonic oxide which is produced and escapes combustion, since the quantity of carbonic anhydride thus diffused through the air is not sufficient in many cases to account for the fatal result. The commissioners, therefore, do not consider the use of water-gas as safe as that of coal or naphtha gas, but they say that the addition to it of petroleum-gas greatly diminishes the danger. So far as they are aware, no accidents have occurred from its use in this country, although there have been several in Europe. The third objection, that the manufacture of water-gas is yet in its infancy, is to a certain extent true, as, although it has been in use in Utica, New York, where the works were recently burned, and is in use in one or two small places, as Poughkeepsie, New York, and the Manayunk District of Philadelphia, it has not been adopted by any of the large companies of Europe or America.

By a comparison of the results obtained by the leading companies in this country and Europe, some interesting facts are shown concerning the cost of production, which, in the United States, has been shrouded until now in mystery—the value of the different processes, the prices charged, etc. The accounts of the London companies, and of the companies of several other large European cities, are published, and therefore open to examination; but, with one or two exceptions, notably of the Philadelphia works, which are controlled by the city, this is not the case with American companies, which are, on the contrary, careful in guarding the secrets of their business. From most of them, certain items of information could be obtained by the commissioners only under the promise of secrecy. The prices charged consumers in Europe are generally much lower than those charged in this country, and it seems that, owing to cheaper labor and better prices obtained for the residuals (coke, tar, and ammoniacal liquor), the cost of manufacture is considerably less. The average price varies with companies and places, from $0.827 per 1,000 cubic feet at London to $1.51 at Paris. The lower price given, however, is charged by only one London company, the S. Metropolitan; the prices of the other companies are much higher, varying from $1.09 to $1.367. The lowest cost of production, 59 cents per 1,000 feet, is reached by the London company just named, and the highest, $1.21, by the Hamburg company. The high cost in Hamburg is to be partly accounted for by the fact that the price of labor is higher there than in any other European city. In 1875 the lowest price in any of the large cities

of this country was in Philadelphia—$2.30. In Boston the price was $2.50, and in New York $2.75; these prices were, however, reduced, in the early part of 1876, to $2.25 and $2.50, respectively. The prices charged in the smaller cities are, as a rule, much higher, being in some cases—Ashland and Bloomsburg, Pennsylvania, for example—as high as $10 per 1,000. In Detroit, Michigan, owing to a temporary war between an old and a new company, the price was as low as 50 cents per 1,000 in one part of the city, and $1 in another; while in a third, supplied only by the pipes of the old company, it was $3.

With regard to the Philadelphia company, which, as before stated, is under control of the municipal authorities, it was found that, while the price charged was the lowest, the cost of the gas was the highest. It had been alleged that the authorities were in the habit of giving employment to laborers for political purposes about election-time, and it was found that the cost of labor was ten cents per 1,000 feet greater than in other works. The proportion of capital to business done varies very greatly with different companies. In Washington, D. C., it was $1.46 per 1,000 cubic feet of gas sold, and in Brookline, Massachusetts, $17.50. "This," remark the commissioners, "must be due, to a great extent, to improper investments or expenditures, and is the great argument against any monopoly being in the hands of a private corporation, and in favor of its management by municipal authorities, since a corporation, having a monopoly, has the power to charge such a price as may be necessary to pay its dividends, and has, therefore, no inducement to diminish its capital, but, on the contrary, one to increase it." The average capital and borrowed money of the London companies, in 1874, was $4.54 per 1,000 cubic feet of gas sold.

As information given them touching the net cost of manufacture was confidential, the commissioners were deterred from publishing the facts as they found them, and forced to resort to giving an approximation. For this purpose, they compare the New York Mutual Company and the Boston Gaslight Company, as being more nearly on an equality, with respect to business done and capital employed, than any others, and deduct the "amount of dividends and taxes *pro rata* per 1,000 cubic feet sold from the average price of gas to the consumer." Each of these companies has an actual paid-in capital of $2,500,000, and bonds to the amount of $500,000. The first paid, in 1875, twenty per cent. dividends on the capital, six per cent. interest on the bonds, and $50,000 taxes, making, in all, $580,000, which, divided among 509,000,000 cubic feet of gas sold, gives $1.14 for each 1,000 feet; this amount, deducted from the net price per 1,000 feet, $2.65, leaves $1.51, which is supposed to be the cost of the gas; and that, the commissioners assure us, is even more than the actual cost. In the same manner the cost of the Boston gas is ascertained to be $1.85 per 1,000 feet, or thirty-four cents more. This difference is

partly due to the use of Albertite instead of naphtha as an enricher, which, as already shown, is more expensive, and partly to greater cost of common coal, labor, and distribution, and smaller receipts for residuals. The leakage, for instance, is only seven per cent. with the New York Mutual, while it is eight and one-half per cent. with the Boston company. The commissioners, however, believe that the gas of this company ought not to cost the consumers more than $2 or $2.10 per 1,000 cubic feet.

Examination of the gas of the New York Mutual showed it to be very pure, and of a high illuminating power. It contained of ammonia about one-quarter of a grain in 100 cubic feet, and less than nine grains of sulphur. Its specific gravity averaged 0.729, and its illuminating power between 20 and 21 candles. The London companies, as before stated, are prohibited by law from allowing the amount of ammonia in each 100 feet to exceed two and one-half grains, and of sulphur twenty grains, and from permitting the candle-power to fall below 16. The amount of ammonia in the Philadelphia gas was found to be 52 grains per 100 cubic feet. "The candle-power," say the commissioners, "is always within the control of the company, and it ought in no case to be allowed to fall below the London standard." In regard to the charges of smokiness made against the gas of the New York Mutual Company, the commissioners, after a thorough investigation, ascertained that where smoke occurred it was due to the ignorance or carelessness of consumers in using unsuitable burners.

All the principal water-gas works were visited, and found to be producing gas of good quality. The cost by the Harkness process, used at New London, Connecticut, exclusive of the cost of labor, purifying, and fuel for the petroleum-retort, which, by this process, requires a separate fire, was found to be about 75 cents per 1,200 feet. The Lowe process, in use in Utica until the works were burned, requires but one fire for decomposing both steam and petroleum, so that it possesses an advantage over the other in regard to the cost of production. The friends of this process claim that, manufacturing at the rate of 200,000 cubic feet per day, the gas can be made (coal being at seven dollars a ton, and petroleum at twelve and one-half cents a gallon) at a cost of 53 cents per 1,000 feet, exclusive of labor, fuel, and purifying. For the Gwynne-Harris process, in use at Poughkeepsie, New York, it is claimed that the cost, under like conditions, is only 37.3 cents per 1,000 feet.

The relation of a gas company to a municipality, the commissioners say, is a peculiar one in many respects, and the company ought not, therefore, to be viewed in the same light with other manufacturing corporations. It supplies a commodity which is not a luxury but a necessity; the sale of its product is a limited one, being confined to the city or district which it supplies; and it is expected to lay its

pipes so that all who desire to burn gas may do so, which entails an expenditure in distribution that is not, perhaps, repaid by the sale of gas in the particular locality for many years; and a very large part of its first investment is in material that would not give any return in case it became bankrupt, or desirous of withdrawing from the business. Therefore it is entitled to a great deal of consideration, provided it performs "its duty to its customers, and is honorable in all its transactions." A general view is then taken of the London companies, the result of their competition, and the efforts which have been made to control them by parliamentary enactments. In the efforts that were made from time to time, between 1820 and 1857, to reduce the price of gas, a number of new companies were chartered and established, until at length thirteen existed, and in some of the streets the mains of three or four companies lay almost in contact with each other. When a leak occurred it was impossible to tell from which main the gas escaped—it was, in some places, impossible to tell with certainty to what company a particular main belonged, and it sometimes happened that a consumer would use the gas of one company and pay for it to another. These circumstances, of course, did not tend to lessen the cost of gas, and so the companies finally agreed to district the city off and abandon competition. Then followed a consolidation of five companies with others, so that only eight remained, and latterly three of these consolidated into one, whereby the number is reduced to six—which result, corresponding as it does with the history of gas companies elsewhere, proves that competition does not operate to reduce the price of gas. It only illustrates the truth of the remark made by John Stuart Mill, and accepted by other political economists, that "where the competitors are so few (as in the case of gas companies), they always end by agreeing not to compete. They may run a race of cheapness to ruin a new candidate, but as soon as he has established his footing they come to terms with him." It eventually ends by the public having to pay the profits on two or more capitals instead of one.

As early as 1820 a committee of Parliament, of which Sir William Congreve was chairman, reported in favor of granting a monopoly under certain restrictions to each company in its own district; but the recommendation was not adopted. As the matter now stands the companies are restricted by law from charging more than 3*s.* 9*d.* per 1,000 cubic feet, and from paying more than ten per cent. dividends on stocks. The law also compels the companies to submit their accounts to the inspection of an auditor, at his pleasure; to publish annual statements of the cost of manufacture, profits, etc.; and it empowers the local municipal authorities to erect works and supply gas if the companies will not agree to sell gas for 3*s.* 9*d.*; and the same authorities may, if they think that ten per cent. dividends can be paid at a less price

than 3*s.* 9*d.*, call for the appointment of three commissioners to reduce the price. And a company may call for a like commission to raise the price if 3*s.* 9*d.* will not pay its allotted dividend.

With regard to the manufacture of gas by municipalities, the commissioners say that the best argument in its favor is, that about fifty cents per 1,000 feet of the gas sold must be applied to the payment of dividends to stockholders, while "a much smaller amount than this, at the low rate at which money could be hired by the city, would be sufficient to pay interest on the capital, and at the same time allow a sufficient amount to be laid aside, in the form of a sinking-fund, to entirely liquidate the debt in a few years." However, "as a rule a city cannot manufacture gas as cheaply as a private corporation, since it is almost impossible to avoid the influence of politics on any city undertaking." Concerning the Philadelphia works, which is the most notable example of municipal manufacture in this country, the commissioners speak as follows: "Notwithstanding all the disadvantages arising from political influence in the management of these works, we find the profits for the year 1875 to have been $793,244.12; and after deducting for interest on the bonds, etc., the sum of $302,986.21 went toward the increase of the sinking-fund, which, on December 31, 1875, amounted to $2,470,193.93, while the whole amount of outstanding bonds is $5,400,000; thus leaving only $2,929,806.07 to be provided for, when the whole works, costing nearly $14,000,000, will become the unencumbered property of the city."

The conclusions arrived at by the committee may be summarized as follows: That although Boston is supplied with gas of excellent quality, at a lower price than most other cities of the United States, the same could be made much cheaper than it is, by the use of naphtha or petroleum as an enricher, but it is doubtful whether the appliances for using that substance could be employed by the Boston company without paying a considerable royalty, or becoming involved in lawsuits for alleged infringement of patents, which are, however, of doubtful validity; that the "Gwynne-Harris" and "Lowe" water-gas processes offer fair prospects of success, and should be carefully watched and studied; that the existing companies in Boston, and other cities in Massachusetts, should be granted monopolies in their several districts, subject, however, to the supervision of a permanent Board of State Commissioners, similar to the Railroad Commissioners, and to a full annual publication of their entire business, and be required to keep their gas at all times up to an illuminating power of sixteen candles, free from sulphuretted hydrogen, and from more than twenty grains of sulphur and five of ammonia, per 100 cubic feet; and, finally, that the authorities of the city of Boston should be empowered by the Legislature to erect works and manufacture and supply gas to the citizens in case any company which, by the terms of its charter, is not subject to legisla-

tive control, should refuse to comply with such reasonable suggestions as may be deemed necessary to insure publicity in regard to its business, proper inspection of its gas, and a limitation of its earnings.

## SKETCH OF PROFESSOR J. P. COOKE, JR.

THE position occupied by this gentleman in American science is one of marked distinction as a successful original investigator, and also as an efficient reformer in the work of scientific education. He is known at home and abroad both by the extent and importance of his experimental researches, and by the high-toned and thorough-going character of his expository works on chemistry and physics. It is a fact of no little significance that, although Prof. Cooke had the advantage of a university training, he was self-taught in chemistry, as his collegiate culture afforded no special preparation for his chosen field of labor, while the impulse which started him in a scientific career came from popular lectures outside the university. Yet he has probably done more than any other man to give chemical science its proper status in the collegiate curriculum as a valuable disciplinary study entitled to a leading place in an effective system of liberal culture.

JOSIAH PARSONS COOKE, Jr., was born in Boston, October 12, 1827, and is a descendant of Major Aaron Cooke, who emigrated from England in 1630, and became one of the first settlers of Dorchester, and afterward of Northampton, Massachusetts. His father, a lawyer, is still living, at the advanced age of ninety, the oldest member of the Suffolk Bar.

Young Cooke received his early education at the Boston Latin School, and entered Harvard College in 1844, where he graduated in 1848. After passing a year in Europe for the recovery of his health, he returned to the university in 1849, as Tutor in Mathematics. He was soon afterward appointed Instructor in Chemistry, and at the close of the following year he succeeded to the Erving Professorship of Chemistry and Mineralogy, which he has held ever since.

Prof. Cooke never had the advantages of a European education, or indeed of any systematic teaching in science. He acquired his taste for chemistry at the early lectures of the Lowell Institute, in Boston, given by the elder Silliman, and, with the apparatus he had collected in a little laboratory in his father's house while a boy, he began his first course of lectures at Cambridge. For several years preceding his appointment no regular instruction in chemistry had been given to the undergraduates, and he had, therefore, the whole labor of developing this department of the college from the begin-

ning. Although scientific schools had been previously established, both at Cambridge and at New Haven, yet Prof. Cooke was probably the first to introduce into our American colleges the experimental method of teaching physical science. He was, at first, greatly hampered by the inflexible recitation system, then universal, and success was only gained after many trials; but Harvard College may now claim to offer its undergraduates as broad and thorough instruction in the various departments of chemistry, including mineralogy, as any similiar institution in the world. Like most American men of science, Prof. Cooke's first duty was to teach, and his time and energy have accordingly been chiefly spent in developing methods of science-teaching, in building laboratories, in making collections, and in providing the various means of scientific instruction.

In connection with his teaching Prof. Cooke has published the following books:

"Chemical Problems and Reactions, to accompany Stöckhardt's Elements of Chemistry," in 1857; "Elements of Chemical Physics," in 1860; "Principles of Chemical Philosophy," in 1869.

In a notice of the last book the London *Chemical News* says: "So far as our recollection goes, we do not think that there exists in any language a book on so difficult a subject as this, so carefully, clearly, and lucidly written;" and in noticing the same book the *American Journal of Science* says: "To Prof. Cooke, more than to any American, is due the credit of having made chemistry an exact and disciplinary study in our colleges."

Prof. Cooke has given many courses of popular lectures in different cities—Lowell, Worcester, Brooklyn, Baltimore, and Washington—besides five courses at the Lowell Institute in Boston. His course of lectures at the Brooklyn Institute, in 1860, was subsequently published under the title of "Religion and Chemistry; or, Proofs of God's Plan in the Atmosphere and its Elements" (1864). In these discourses he aimed to show that the argument for design is not invalidated by the theories of evolution.

A course of lectures on electricity at the Lowell Institute, Boston, in the winter of 1868–'69, was followed by the publication, in the *Journal of the Franklin Institute*, of a series of papers on the "Absolute System of Electrical Measurements" and on the "Theory of the Voltaic Battery." In the last he developed a new theory of electricity, which has also been embodied in the later editions of his "Chemical Philosophy." This theory, like that of Dufay, admits two electrical fluids; but it regards these as separable constituents of the ether of space. These ethereal fluids, more or less blended, form an atmosphere around every molecule held in place by the immense force of molecular attraction; and when, by the various causes of electrical disturbance, the electrical ethers become more or less isolated on the same or on different molecules the two tend to flow

together with great rapidity in virtue of their wonderful elasticity. If we assume that this motion takes place in accordance with the well-known laws of the diffusion of gases, the theory gives a satisfactory explanation of all well-established electrical phenomena.

In the autumn of 1872 Prof. Cooke delivered an interesting and important course of lectures on the "New Chemistry," which was subsequently published in the "International Scientific Series." His volume is one of the best and most successful of these books, and has been very highly appreciated both in this country and abroad, having been translated into most of the languages of Europe.

Among Prof. Cooke's lesser scientific publications may be mentioned the following:

1. "On the Relation between the Atomic Weights of the Chemical Elements" ("Memoirs of the American Academy," vol. v., 1854).

It was first shown in this paper that when the elementary substances are classified in natural groups, their atomic weights and other physical qualities are related by regular differences.

2. "On Two New Crystalline Compounds of Zinc and Antimony, and on the Cause of the Variation of Composition observed in their Crystals" ("Memoirs of the American Academy," vol. v., 1855).

This investigation proved that the crystalline form of these compounds was preserved under very considerable variations of composition, and indicated that the excess of one or the other constituent depended not solely on the composition of the menstruum in which the crystals were formed, but also on the chemical force which determines the union of the elements in definite proportions. The subject was still further discussed in the following paper, published during a visit to England.

3. "Crystalline Form not necessarily an Indication of Definite Chemical Composition, or on the Possible Variations of Composition in a Mineral Species independent of the Phenomena of Isomorphism" (*Philosophical Magazine* for June, 1860).

4. "On the Dimorphism of Arsenic, Antimony, and Zinc" (*American Journal of Science* for March, 1861).

It was here proved that all three of these elements are capable of crystallizing in octahedrons of the regular system.

5. "On Octahedral Galena" (*American Journal of Science* for January, 1863).

In this it was shown that the octahedral cleavage in this singular variety of galena from Lebanon County, Pennsylvania, is merely an unusual development of a constant condition.

6. "Crystallographic Examination of the Hebron Childrenite, and Comparison of this Variety with the Childrenite of Tavistock" (*American Journal of Science* for September, 1863).

7. "Crystallographic Examination of the Acid Tartrates of Cæsia and Rubidia" (*American Journal of Science* for January, 1864).

8. "On the Projection of the Spectra of the Metals" (*American Journal of Science* for September, 1865).

9. "On the Construction of a Spectroscope with a Number of Prisms by which the Angle of Minimum Deviation for any Ray may be accurately measured" (*American Journal of Science* for November, 1865).

10. "On the Heat of Friction" ("Proceedings of the American Academy," 1865).

11. "On the Aqueous Lines of the Solar Spectrum" ("Proceedings of the American Academy," 1866).

By comparing observations with the spectroscope and the hygrometer, it was in this paper first shown that a large part of the air-lines in the solar spectrum are due to aqueous vapor.

12. "On Danalite, a New Mineral Species from the Granite of Rockport, Massachusetts" (*American Journal of Science* for July, 1866).

This is a well-marked species allied to Helvin, but containing zinc in place of manganese.

13. "On Cryophyllite, a New Mineral Species of the Mica Family, with Some Associated Minerals in the Granite of Rockport, Massachusetts" (*American Journal of Science* for March, 1867).

Besides establishing a new species, this paper shows that in the veins of the Rockport granite there are closely associated a unicilicate and a bisilicate mica, which are isomorphous with each other, a circumstance which renders probable the theory that the wide variations in the composition of the micas may result from an isomorphous mixture of two similar types.

14. "On Certain Lecture Experiments, and on a New Form of Endiometer" (*American Journal of Science* for September, 1867).

15. "A Method of determining the Amounts of Protoxide of Iron in Silicates not soluble in the Ordinary Mineral Acids" (*American Journal of Science* for November, 1867).

16. "Crystallographic Determination of Some American Chlorites" (*American Journal of Science* for September, 1867).

The paper gives some new measurements of angles, and shows that there are two crystallographic types of chlorites corresponding to the well-known types of micas. It is further shown that there is a variation of optical angles in the chlorites, even on the same specimens, like that observed with the micas, and the inference is drawn that the variation is due to a similar cause.

17. "Atomic Ratios" (*American Journal of Science* for May, 1869).

It was for the first time pointed out in this paper that what mineralogists have long called the oxygen ratio of a silicate is really the ratio between the atomicities of the acid and basic radicals in these salts.

18. "The Vermiculites—their Crystallographic and Chemical Relations to the Micas, together with a Discussion of the Cause of the Variation of the Optical Angle in these Minerals" ("Proceedings of the American Academy," 1873).

This monograph contains the chemical analyses and crystallographic descrip-

tions of several micaceous minerals which are here classified together, and shows that the variations of the optical angle in the micaceous species is due to the interfoliation of the different members of a macle. It also points out the close relation between hexagonal and trimetric crystals by showing that a hexagonal form and structure may result from a similar macling of trimetric crystals the prismatic angle of which is 120°.

19. "Melanosiderite, a New Mineral Species from Mineral Hill, Delaware County, Pennsylvania" ("Proceedings of the American Academy," 1875).

20. "On Two New Varieties of Vermiculites, with a Revision of Other Members of this Group," published in connection with F. A. Gooch ("Proceedings of the American Academy," 1875).

21. "On a New Mode of manipulating Hydric Sulphide" ("Proceedings of the American Academy," 1876).

This in application of the soda-water fountain, by which hydric sulphide is dissolved in water under pressure, and the magnet readily applied in a concentrated form.

22. "On the Process of Reverse Filtering, and its Application to Large Masses of Material" ("Proceedings of the American Academy," 1876).

This enumeration occupies but a small space; but when it is considered that each paper only states the results of elaborate and protracted original and experimental investigation, where the unverified guesses and the trials that go for nothing do not appear, we can form some idea of the amount of labor involved in the quiet life of a true scientific man.

Prof. Cooke has also written various articles for encyclopædias and reviews, and published several addresses. His discourse on "Scientific Culture," delivered at the opening of the summer courses of instruction in chemistry, at Harvard University, July 7, 1875, printed in THE POPULAR SCIENCE MONTHLY for September, 1875, and republished in London, was one of the ablest contributions to the literature of scientific education that have appeared in a long time. Prof. Cooke's life has been one of valuable scientific service, which has, moreover, met with wide and cordial appreciation. He has been honored by the membership of many learned societies in this country and in Europe, and was quite recently elected foreign honorary member of the Chemical Society of London.

# EDITOR'S TABLE.

### *HONOR TO ADAM SMITH.*

ABOUT a hundred gentlemen sat down to dinner at Delmonico's, December 12th, in commemoration of the centennial anniversary of the publication of Adam Smith's "Wealth of Nations." The occasion was an interesting one, and the various topics suggested were treated with an earnestness and ability of which the public got but a very imperfect idea through the newspaper reports. Mr. Parke Godwin presided with efficiency, and made a very instructive opening speech, which was followed by addresses from Mr. Bigelow, Mr. Atkinson, Mr. D. A. Wells, Prof. Sumner, and Dr. Anderson, of the Rochester University, each of which brought out an important aspect of the great subject of free-trade. While Adam Smith was honored as the chief historic representative of rational and liberal views in regard to the liberty of commerce, it was pointed out that his position may be easily misconceived, and his claims exaggerated. Without denying the proposition of Mr. Buckle, that Smith's "Inquiry into the Nature and Causes of the Wealth of Nations" is probably the most important book in its influence upon the policy of states and the economical welfare of mankind that was ever written, it was shown also that Adam Smith was but the mouthpiece of his age; that a preceding generation of inquirers had prepared for him; that the French economists were in advance of Europe in their economic views; and that an elaborate French work appeared in 1776, simultaneously with the "Wealth of Nations," in which the same conclusions were reached, and enforced with great clearness and power. As stated by Mr. Bigelow, it was but another case so common in the progress of scientific investigation, where the ideas reached belong rather to the epoch than to any individual exponent of them.

Mr. Wells gave an admirable account of the workings of the restrictive system, which burdened the industries of Europe from the middle ages down to the time of Adam Smith. He showed that the fundamental idea in all business transactions, whether between nations or individuals, was that parties trading were in necessary relations of enmity, and that what one man or one nation gained the other party inevitably lost. So radical was this antagonism regarded between men, guilds, and different countries, as to find expression in Hobbes's theory that the state of man in society is one of necessary and perpetual war. The merit of Adam Smith was, that he demonstrated the utter fallacy of this view, and proved that by the natural laws of trade the advantages of exchange are mutual, and that in its largest possible freedom there will accrue the largest possible benefits to all. Christianity had been trying for many centuries to enforce the golden rule of mutual justice as a matter of duty, to be carried out even though it involve suffering and loss; Adam Smith showed that the rule of right in human intercourse, so far as trade is concerned, produces reciprocal good, and is for the pecuniary interest of both parties.

Dr. Anderson maintained forcibly and impressively that free-trade is to be placed on the broadest grounds of morality. The liberty of commerce he held to be a God-given right as much as any other kind of liberty; and the restrictions upon trade to be just as immoral and vicious as interference with other forms of freedom. A man

has a natural and inalienable right to his personal freedom of action, to the use of his muscles, and the employment of his powers, in any manner and direction that he chooses; to contravene this is slavery. A man has a right to the use of his mind, to freedom of thought and speech; and to interfere with this is tyranny. A man also has a natural right to freedom of exchange of the products of labor, to buy and to sell, as he pleases and where he pleases; and every arbitrary impediment to this liberty is despotism. In the advance of civilization, and through the struggles of ages, personal liberty of action and thought has been secured; but it still remains to extort from governments absolute freedom of commercial intercourse, whether on a small scale or large. Dr. Anderson paid a compliment to the great abstract thinkers—Grotius, Smith, and Bentham—who, although only scholars and philosophers, have exerted a powerful influence upon the modern world; and he stated that free-trade doctrines are now taught in all our best colleges so efficiently that this influence will be certain to tell in the future settlement of the question.

Prof. Sumner took up, briefly, the present state of political economy, and remarked upon its incompleteness and the conflict of views that has recently sprung up in regard to its scope, its validity, and its permanence. While many of its questions will have to be further elucidated, while much that was at first laid down as true has required revision, and while other forms of knowledge are reacting upon and modifying it, Prof. Sumner is of the opinion that political economy must stand in the future as an established division in the classified hierarchy of the sciences.

Mr. Sanborn, of Boston, followed this line of thought in some remarks on the relation of political economy to social science. In that closer interdependence of the various forms of knowledge which has resulted from scientific investigation our views become enlarged, and it is apparent that these subjects must more and more be considered together. Political economy will suffer if studied exclusively, or with no reference to that philosophy of man and society of which it is but a part.

Dr. Leverson closed the speech-making by an appeal to introduce the study of the rudiments of political economy in our schools. He testified, from his own large experience as a lecturer, both in England and in this country, that pupils in schools may be very early interested in an elementary knowledge of economics, or of the sources of familiar things and the business operations by which they are procured. He thought this was the proper place to begin the study of social science.

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### *SOCIETIES FOR THE DIFFUSION OF SCIENCE.*

The necessity of associated action for the attainment of desirable and important public objects is generally understood, as is shown by the numerous societies and organizations for the promotion of religious, political, philanthropic, literary, historical, and scientific objects. The directions taken by such associations in respect to the interests to be promoted are, of course, various, and well represent the state of intelligence, the culture, the mental preoccupations and aspirations, of the community in which such societies are formed. As regards science, the organization of societies for its promotion has mainly had for its object the encouragement and aid of original observation and research; and, as men devoted to independent inquiry are not numerous, and are widely scattered, such associations are neither large in number nor strong in their member-

ship and support. Moreover, from the nature of their objects they are more completely cut off from public interest, sympathy, and patronage, than any other societies. In speaking of the meeting of the American Association for the Advancement of Science, held at Buffalo last August, we called attention to the duty of scientific men to take the public more into the account in the organization of their work, and we showed how that might be done without any detriment to the proper objects which the convention had before it. Of all the subjects that are now promoted by social combinations, that of the *diffusion* of science owes the least to such agencies. The work of disseminating scientific knowledge among the people goes slowly on, by means of the press, by schools, and by lectures; but it would be much more vigorously prosecuted if it were made a distinctive and prominent object, either in associations expressly formed for the purpose, or in societies that combine different lines of effort in the general purpose of popular instruction. It is gratifying to note the multiplication of scientific academies in the leading cities of the country, which bring together observers and investigators, and call out original contributions that prove to be valuable and worthy of publication in an annual volume of "Transactions." But such associations can only be sustained at the larger centres of population, and even there they must struggle hard to maintain their existence. But if these bodies embraced within their plans, as a leading and permanent object, the diffusion of science through the community and the scattering of valuable information upon practical subjects, there can be little doubt that they would be better sustained, both by attendance at their meetings and by the contribution of funds to carry on their operations. Moreover, in the smaller cities and towns, where the higher work of science is impracticable from the fewness of its cultivators, societies promotive of popular scientific education might be created that would do efficient and valuable service. Scientific libraries might be collected, scientific essays contributed, and followed by instructive discussions, and courses of lectures secured from competent men on subjects that would enlist the attention and secure the liberal patronage of the public. In every town of five or ten thousand inhabitants a dozen active, thoughtful, and spirited men might be found, competent to organize and manage such a society, that would effect much good in the locality; and, if adjacent towns did the same thing, much might be gained in various ways by coöperation. Only one thing is needed to achieve this result, and that is, a hearty interest and some enthusiasm in the enterprise on the part of a few individuals to carry it on.

We by no means claim that such an association should be exclusively scientific in its aims. It might embrace literature, local history, political economy, and various social questions, among its objects. We only urge that the popular diffusion of scientific information should be an essential element and a clearly-recognized object. From such modest and perhaps ill-defined beginnings valuable and lasting institutions have often arisen. We have met with some remarks in a paper on the "Historical Societies of the United States," contributed to the Report of the Bureau of Education in Washington by Dr. Henry H. Holmes, Librarian of the New York State Library at Albany, which are so suggestive in relation to this subject that we take the liberty of quoting them:

"To these observations on the question of enlarged plans for local societies, we venture to subjoin the further inquiry, whether most county and town societies might not, with incalculable advantage, combine with

historical research the study of science, art, and natural history? Every locality already has its military, fire, debating, literary, social, or charitable society. It is incredible that there should be so few simply for the pursuit of knowledge to the acquisition of which all men are so naturally impelled and in which they manifest so deep an interest. The same motives, which dispose some of the leading minds of a place to associate for the sake of preserving its history, must be operating in the minds of others, their neighbors, to desire to acquire and communicate knowledge in other forms. On the part of those interested in history it should be regarded as a strong reason for extending the scope of their society, the consideration that when confined to a single subject it will depend for its permanence on the activity of two or three members. It does not afford a basis sufficient for the active cooperation of more than a small portion of the cultivated minds of the place; the topics either soon become exhausted as matters of continual research, or the information is meagre and accumulates slowly, and the popular interest diminishes; the meetings cease to be attended, and the society either dies of inanition or languishes while standing in the way of a new organization on a more comprehensive plan.

"It may be urged as an objection that some of our societies have commenced with the title of 'historical and philosophical,' and have not been remarkably successful. Others, however, have tried the plan of conjoined aims, and congratulate themselves on the result. The Essex Institute of Salem, Massachusetts, was formed in 1848 from the union of a county historical and a county natural history society, and organized on a popular basis of large membership, having at the present time four hundred and eighty members. With the aid of historical and scientific workers it is prosecuting both branches with an efficiency, as shown by its publications, which must compel imitation. The Albany Institute, New York, has been perpetuated with varying fortunes for forty-six years, and has four departments of research—physical science and the arts, natural history, history, and general literature. It has at no time been so promising an organization as at the present, when it has been extended to a membership of two hundred and four. A similar successful society is the Literary and Philosophical Society of Liverpool, England, founded in 1846, which has over two hundred members, and has published twenty-eight volumes of its 'Transactions.' The subjects treated of in these conform, in fair proportion of literature, history, and science, to the name of the society. One motive assigned in its constitution for organizing the society, 'to modify the local tendency to the pursuit of commerce,' is capable of receiving a wider application.

"We have purposely alluded to the large membership in these three societies, because a late scientific writer, speaking of the frequent failures of the learned societies of the United States, declares that they have died from 'a constant enlargement of the range of membership, and consequent lowering of the tone of the society' (*North American Review*, October, 1874). And yet we draw from this same writer the two facts that the membership of the leading English societies ranges from four hundred to one thousand or several thousand members, and that the annual tax on each member is from two to four guineas. We should infer from these facts that, by a large membership, an abundant income is secured for the purposes of a society, and that the original papers of the men of science who are joined with them can be published, and the expense of their investigations provided for. A large membership secures friends, an audience, an income, and elevates the purposes and aims of all. Some aid by active efforts, some by pecuniary help, and all by the sympathy of a common purpose. Membership is not a reward of merit, acquired for achievements in literature or science, but an encouragement and a stimulus both to the less learned and the most learned. It ought not to be difficult to combine the man of research with the intelligent aspirant for knowledge, who educates himself for similar researches by means of the companionship. To the man of science or invention it must be desirable that he should have the encouragement of a listening audience, and be brought in contact with men of varied pursuits, outside of his specialty. It affords him an opportunity at least to utter his words of scientific truth before his fellow-citizens. To make an addition to the sum of human knowledge, or to diffuse and inspire a love of it, may be of equal importance to humanity.

"In suggesting this combination of varied objects of pursuit, we are not, of course, supposing that academies of scientists can be founded everywhere; but we cannot resist the belief that in most counties and towns there will be found a sufficient number of men of education, of all professions, occu-

pations, and opinions, disposed to unite for the mutual pursuit of history, science, and the arts; and that they will engage in it, not in a spirit of exclusiveness, but of benevolence, aiming to develop a love for the most elevated and accurate forms of knowledge. It should be easy, in a multitude of places, for associations formed with these blended purposes to sustain twice a month, or even weekly, during a large part of the year, meetings for the purpose of listening to papers, original or compiled, from members or invited speakers, or for the discussion of any topic introduced. By some such method as this, local societies would become schools of thought and learning for the active members of the community in hundreds of our towns and cities. There might naturally follow a union of the societies of a State under a general society, for the publication of such papers as might be deemed suitable.

"The extensive formation of such societies throughout the land seems so full of promise and so potent for good as to justify the establishment of a national society for the organization of associations for the pursuit of knowledge. Such a society might initiate efforts which would have the cordial support of co-workers in every State of the Union. The original name of our oldest learned society, the American Philosophical, of which Franklin was the first president, was 'The American Society for Promoting and Propagating Useful Knowledge.' The title is an indication of the expanded and benevolent designs of its founders. This society had, also, its standing committee on history and commerce. If the Smithsonian Institution, founded 'for the increase and diffusion of knowledge among men,' should be able to incorporate, with its present benefactions to science, the support of an agency for encouraging such societies as have been described, it might be hoped it would not be a departure from the spirit of its founder. It would be an agency, by whatever association it should be controlled, for introducing and promoting a plan for enlisting tens of thousands in the direct study of science, art, and history. Such societies would be the means of educating many communities to a loving appreciation of scientific investigations, and of correct views of human history. They would contribute incalculably to the progress of American society and to the happiness of millions."

## LITERARY NOTICES.

ELEMENTS OF PHYSICS, OR NATURAL PHILOSOPHY. By NEIL ARNOTT, M. D., LL. D., F. R. S. Seventh edition, edited by ALEXANDER BAIN, LL. D., and ALFRED SWAINE TAYLOR, M. D., F. R. S. New York: D. Appleton & Co. Pp. 873. Price, $3.

WE are glad to see this sterling and favorite work brought up to date, as it is in the edition now issued. A generation ago Arnott's "Physics" was the leading text-book on natural philosophy both in England and this country, and we much question if for educational purposes anything equal to it has appeared since. We have physical text-books with finer pictures, but we have gone to an excess in this direction, and greatly overdone the pictorial element. It is an objection to large, elaborate, and profuse illustrations, that they are costly, that they trench upon the text, and often give prominence to trivialities, simply because they afford an opportunity for a showy engraving. The illustrations of a high-grade scientific book should be simple, and severely subordinated to the ideas they exemplify. The cuts in Dr. Arnott's book, while having no merit as mere pictures, are perfectly sufficient for their purpose of illustration.

There is another objection to our recent text-books of physics in the want of balance or proportion in treating of subjects. The rage for the new, and what is called keeping up with the times, has led to undue prominence in representing the last results of science, and to a corresponding neglect of those established facts and principles which have lost their novelty because they are old and well-settled. A book filled with the recent wonders of research may be exciting, and full of interesting information, but these qualities cannot commend it to students whose object is to acquire the body of principles that constitute a science. In this respect, and in physics especially, the value of the old greatly preponderates over that of the new. No doubt such works should be up to date, and represent "the present state of science," but facts discovered a great while ago, and long-determined laws, are quite as much parts of the present state of science as the last results of in-

quiry. In this respect, also, Arnott's "Elements of Physics" is more harmonious and well-proportioned than many of the later works upon the subject. Its careful editors have brought it up to date by introducing clear accounts of the various advances in physics that have been made during the last twenty years. The modern doctrines of Energy, Correlation of Forces, the Mechanical Theory of Heat, the Kinetic Theory of Gases, Barometric Gradients, Weather Areas, and Storm-signals, Tyndall's and Helmholtz's Acoustical Investigations, Spectrum Analysis, the Radiometer, and many other results of research in recent years, are all introduced in their appropriate places, and briefly and succinctly explained. But they fall into their proper relation as but a small part of the great system of truths that must now be comprised in any standard treatise upon the science of physics. The editors, we observe, have caught the spirit of the work, and assimilated the new matter to the method of exposition adopted by the author.

And it is in this that the unrivaled merit of Dr. Arnott's work chiefly consists. The style in which it is written, as is well known, is a model of easy simplicity. It is the most *readable* book on natural philosophy that we have in the language. Another admirable feature is the copiousness and diversity of its illustrations and concrete applications of physical principles. These are mainly drawn from the familiar field of every-day life, and, notwithstanding the numerous books that have appeared on common things, familiar science, etc., Arnott's "Physics," is still our best book of this kind. He has been much copied, but his statements have not been improved upon. The new edition of this work may therefore be strongly recommended to schools as a text-book, a reference-book, or a reading-book, and, however used, it will be pretty sure to do good service.

MODERN PHYSICAL FATALISM, AND THE DOCTRINE OF EVOLUTION, INCLUDING AN EXAMINATION OF MR. HERBERT SPENCER'S "FIRST PRINCIPLES." BY THOMAS RAWSON BIRKS, M. A., Knightbridge Professor of Moral Philosophy, Cambridge. New York: Macmillan & Co. Pp. 311. Price, $2.25.

WE have here another attempt to demolish Herbert Spencer, and it is noteworthy chiefly as emanating from a dignitary of the University of Cambridge. Those in quest of objections to Spencer's system, and not very particular about their quality, will find in this volume a great deal of material adapted to their purpose. But as a polemic it is by no means equal in subtilty, force, or originality, to various replies to Spencer that have previously appeared. In our judgment, it is quite inferior in logical acuteness to Prof. Bascom's criticism in the *Bibliotheca Sacra* of last October, while in candor, courtesy, and philosophic liberality, the English author is not for a moment to be compared to the American reviewer. The book is dominated by an intense theological bias, and is written from the standpoint and in the interest of the most unmodified type of orthodoxy. The author writes in behalf of such vast interests that he cannot be trusted. Absorbed in the interests of the eternal world, he is lax and careless about the things of this world—does not represent them as they are. In the first chapter, and on the very first page, he says that in Spencer's system of thought "science is identified with physics," and that this is the way he reconciles religion and science. This, of course, is absolutely false, and not only so, but it is a misrepresentation so fundamental as to taint the work through and through. A writer who would commit so flagrant a misrepresentation at the threshold of his work forfeits at once his claim to the confidence of intelligent readers, who will see that a discussion so vitiated and loosely carried on is not worth pursuing.

Dr. Birks makes wholesale objections to the doctrines of the Unknowable, the Relativity of Knowledge, the Indestructibility of Matter, the Conservation of Force, Evolution and Natural Selection, and closes his book by saying: "The doctrine of the Unknowable is a lower depth in the scale of intellectual and spiritual darkness than the old Athenian idolatry. The Persistence of Force, and the Indestructibility of Motion, when set up to replace the true and living God of the Bible, the Almighty Creator of heaven and earth, will be found on inquiry to be still meaner and more worthless than the old heathen idols of wood and stone. One sentence of the Word of God, in the song of the heavenly elders, lays the foun-

dation of a philosophy nobler and deeper than all the human counterfeits of these latter days."

WINDS OF DOCTRINE: being an Examination of the Modern Theories of Automatism and Evolution. By CHARLES ELAM, M. D. London: Smith, Elder & Co. Pp. 163.

THIS is a work of a similar stripe to that just noticed. The contents of the volume first appeared in the *Contemporary Review*, in three articles, and coming from a medical man, the presumption should be that it is a scientific discussion, but it is rather a piece of violent rhetorical denunciation. The author contributes nothing to the scientific illumination of the subject, and takes his cue from some of the outgivings of Prof. Mivart in his recent criticisms of Darwinism. But while Mr. Mivart, like most of the eminent biologists of the time, admits evolution as a great historic fact of Nature, however deficient may as yet be its explanation, Dr. Elam scouts it in every form and degree as a pure figment of the imagination, and an idle absurdity. His virtual position is, that the naturalists are under an hallucination, and that Darwin, Huxley, Tyndall, and Spencer especially, to whom he gives his main attention, are little better than fools so far as *this* subject is concerned. Like Prof. Birks, Dr. Elam writes in the interest of popular traditions and for miscellaneous readers, and has no scruple about his course so he can make out a specious case. He quotes Huxley copiously, but prefers to use his cautious statements, made twelve or fifteen years ago, rather than his later utterances which represent the progress that has been made within that time. With equal unfairness he goes back to Spencer's "Social Statics," published twenty-six years ago, and quotes opinions which Mr. Spencer has stated that he now holds only with important qualifications, instead of judging him by the work upon the same general subject that he is now elaborating.

The spirit here evinced is that of the advocate and partisan, rather than of the candid and earnest inquirer after truth.

There are difficulties with evolution, many, and various, and formidable; and none better understand this, or more freely acknowledge it, than those who have studied the subject most profoundly. There are not only inherent difficulties in the discussion from imperfect knowledge, but there are extrinsic difficulties in bringing before the general mind the nature and force of its proofs, and from its conflict with long-established and widely-cherished beliefs. It is therefore a perfectly easy thing to make objections to the doctrine which many will think annihilating. It is an easy thing to accumulate and ring rhetorical changes on old objections, and with a little license of misrepresentation, and a fresh battery of depreciatory adjectives, to make out a killing case in the estimation of those whose minds are made up beforehand, and who know little of the real issues of the subject. If, on any plain and simple question, arising out of an open transaction between two neighbors who have become involved in law, the hireling attorneys can so confuse and confound all common-sense that a jury is as likely to give a wrong verdict as a right one, what may we not expect when a great, complex, wide-reaching, and newly-presented scientific question becomes a matter of controversy before ill-instructed people, with loud and angry protestations that it involves the very existence of morality, religion, and God? The skillful counselor, who cares only to produce an impression, has obviously a great advantage here.

But while the pert and supercilious critic is carrying all before him, and proving to those who knew it all before that evolution is a baseless fancy, a mere transient gust of wild and absurd speculation, the disciplined, sober-minded, and thoroughly-instructed naturalists, guided by the light it affords, are penetrating deeper into the secrets of phenomena, making further discoveries, and rapidly extending the bounds of our knowledge of Nature.

INVENTIONAL GEOMETRY. A Series of Problems intended to familiarize the Pupil with Geometrical Conceptions, and to exercise his Inventive Faculty. By WILLIAM GEORGE SPENCER. With a Prefatory Note by HERBERT SPENCER. New York: D. Appleton & Co. Pp. 100. Price, 50 cents.

THIS is a small and a modest book, but a very important one for all who have a concern about the quality and character of education. It is not a book that will work

well in our smooth-running educational system—not a machine that can be belted on in some convenient corner of our educational cotton-mills. In the objects it aims to secure, and in the method of attaining them, it is outside of the customary school routine. It is a contribution to the momentous and much-neglected work of self-education to which the school-room, as commonly managed, is not very favorable.

The author of this little work, the late W. G. Spencer, of Derby, England, was a mathematical teacher, and a gentleman of wide cultivation and independent opinions. He had been for many years an instructor, and entertained quite unconventional views on the subject of education. He maintained that, of all that passes under the name, only that is truly education which calls out mental exertion, trains the pupil to the exercise of his own faculties, develops the judgment, and gives the student the ready use and command of his own mind. It is, therefore, necessary constantly to throw the pupil back upon himself, and, while encouraging and guiding him, leave him at the same time to do his own work. For the usual occupations of the school-room, explanatory instruction, loading the memory with the contents of books, and helping the pupil rapidly along by all kinds of facilities and devices, Mr. Spencer had but little respect; and he measured the excellence of the teacher by his faculty and resources for awakening the pupil's interest, keeping him judiciously occupied, and inciting him to use, cultivate, and strengthen his own powers.

In the later years of his teaching, Mr. Spencer was much occupied in giving private instruction in mathematics, and he was therefore brought into constant contact with individual minds, and enabled to study them much more critically than if he had been dealing with classes in the usual way. In these circumstances he devised a course of exercises in elementary geometry for the use of beginners, designed to lay the foundation for mathematical study, and at the same time to cultivate the faculties of invention and construction, which are of the highest importance, and almost totally neglected in the common methods of the schools. As this course of exercises began with the simplest problems, and was skillfully graded so that the pupil could do the whole work himself, there seemed to be no reason why the benefits of the method should not be extended to all who might wish to avail themselves of it, and it was so highly appreciated by those who had used it that the author was at length induced to print it, although he had no such intention at the time of its preparation. The "Inventional Geometry" is now republished, and being a very suitable book for companionship with the "Science Primers," now being issued from time to time, the publishers have thrown it into the same form, and included it among the reprints in this elementary series. But in one respect the "Inventional Geometry" differs from the little books with which it is associated. The Science Primers are highly estimated. They fall in with the stereotyped habits of the class-room, and may be easily learned by heart like history, grammar, or the catechism. This book will, however, enforce a different treatment, and if the object of education be the discipline of the mental faculties through honest effort, and if thorough familiarity with geometrical conceptions be desirable, and the training of the inventive and constructive faculties be valuable and important, the Primer of Geometry will be worth more than all its associates put together. We recommend it to those who are thoughtful and conscientious in educational matters. Any fair-minded boy or girl of twelve or fourteen years of age can go through it without difficulty, and cannot get through it without gaining the advantages it aims to secure. Those who work their way through it will be certain to know one thing thoroughly, and, as Goethe said to Eckermann, "It is always an advantage to have any clear bit of knowledge."

The author of this book was the father of Herbert Spencer, who testifies from observation and experience to the excellence of the method, as will be seen by the following note to the publishers:

"LONDON, *June* 8, 1876.

"MESSRS. D. APPLETON & CO.: I am glad that you are about to republish, in the United States, my father's little work on 'Inventional Geometry.' Though it received but little notice when first issued here, recognition of its usefulness has been gradually spreading, and it has been adopted by some of the more rational scienco-

teachers in schools. Several years ago I heard of its introduction at Rugby.

"To its great efficiency, both as a means of producing interest in geometry and as a mental discipline, I can give personal testimony. I have seen it create in a class of boys so much enthusiasm that they looked forward to their geometry-lesson as a chief event in the week. And girls initiated in the system by my father have frequently begged of him for problems to solve during their holidays.

"Though I did not myself pass through it—for I commenced mathematics with my uncle before this method had been elaborated by my father—yet I had experience of its effects in a higher division of geometry. When about fifteen, I was carried through the study of perspective entirely after this same method: my father giving me the successive problems in such order that I was enabled to solve every one of them, up to the most complex, without assistance.

"Of course, the use of the method implies capacity in the teacher, and real interest in the intellectual welfare of his pupils. But given the competent man, and he may produce in them a knowledge and an insight far beyond any that can be given by mechanical lesson-learning.

"Very truly yours,

"HERBERT SPENCER."

THE GEOGRAPHICAL DISTRIBUTION OF ANIMALS, with a Study of the Relations of Living and Extinct Faunas as elucidating the Past Changes of the Earth's Surface. By ALFRED RUSSELL WALLACE. Two vols. With Maps and Illustrations. New York: Harper & Brothers. Pp. 1110. Price, $10.

THIS work has grown out of the recent progress of biological science, and could neither have been produced earlier than it has been, nor probably by any other living author. To those who regard the evolution hypothesis as a piece of mere useless speculation, it may be replied that it is the most powerful stimulus to investigation in the higher science of living things that has yet been known, of which the noble work before us is incontestable proof. The problem of animal distribution is here so conceived and presented as to give it very much the character of a new subject.

Up to this time, a naturalist has only needed to try to learn about the fauna of any country to be made aware of our lack of knowledge in this field. Much has been learned, of course, but the records were fragmentary and scattered, and it was only on the shelves of the best zoölogical libraries that anything approaching completeness was to be found, so that practically such information has been inaccessible. But with the growing interest in Darwinism there came an appreciation of the value of the study of distribution, and a demand arose which made itself felt. As is always the case, the demand only needed to become urgent to insure a supply. And it was to meet this want, growing daily more pressing, that Mr. Wallace put forth this work—and the task could not have fallen into better hands. His life has been one of preparation for it. As early as 1848 he embarked with Mr. H. W. Bates for the Amazons, and in that region—the richest in animal life—and later in the Malay Archipelago, the best years of his life were given to the study of zoölogy. Few of our readers need to be reminded that in those far-away lands he independently worked out the theory of "natural selection." The more difficult work of establishing the validity of the "doctrine of descent" fell into other and, as Mr. Wallace modestly and gracefully says, abler hands; but he has not ceased to work in that field, and has given great aid in searching out relevant facts and showing their bearings. This work is certainly one of the most valuable of these contributions. From the scattered sources he has, with infinite pains, collected the details of that which was known, and, arranging them with a skill and method which leave little to be desired, has put them within the reach of all.

The book sets out with an introductory chapter, showing the inadequacy of the popular notion that the manner in which animals are dispersed over the globe is due to diversities of climate and vegetation. Much as there undoubtedly is to give rise to this belief, a little examination shows that no such off-hand treatment will do. That South Africa has lions and giraffes, and Australia kangaroos and other marsupials, finds no explanation in differences of soil and climate, because no marked differences exist. So, too, the theory fails when we find Europe destitute of raccoons, opossums, and humming-birds, and North America without hedgehogs or true fly-catchers, although the conditions of life are in all essentials similar in the two regions.

Assuming the view that each species

has had one birthplace, and only one, the second and third chapters discuss the means by which dispersal has been effected, and what bearing the surface-changes of the earth have had on distribution. They are of great interest, and admirable examples of the efficiency of scientific induction when applied by able hands to the solution of perplexing problems.

The principles upon which zoölogical regions should be formed are next considered, and the reasons given which led the author to adopt, with little change, the divisions proposed by Mr. P. L. Sclater in 1857, which maps the globe into six great primary regions, the Palæarctic, Ethiopian, Oriental, Australian, Neotropical, and Nearctic.

Zoölogical classification receives, as of course it should, due consideration. Mr. Wallace attempts no reconciliation of the disputed points of classification, but selects and tabulates for his uses a few of the best-known classes. As the title-page indicates, the relation and distribution of extinct faunas have an important place. The recent lectures of Prof. Huxley are too fresh in the minds of our readers for it to be necessary to emphasize the value of the study of fossil forms in connection with the general doctrine of evolution. In the hands of Mr. Wallace its application to the question of distribution is full of suggestion and interest. We may add that, in this connection, due acknowledgment is made of the successful and important labors of American paleontologists.

In Parts IV. and V. are treated, first, the forms of life as seen in the different zoölogical regions, their differences and resemblances being pointed out; with, lastly, a systematic, tabular arrangement of the families of the animals considered, and sketches of their geographical distribution. The value and interest of these volumes are enhanced by a series of twenty plates showing the physical aspect and special zoölogical character of the different sub-regions, and by a set of excellent maps on which are shown the outlines of the regions and sub-regions, the belts of altitude, the forests, pastures, deserts, and snow-lines, together with the contours of the beds of the great oceans as determined by the most recent soundings.

FURTHER NOTES ON "INCLUSIONS" IN GEMS, ETC. By ISAAC LEA. Philadelphia: Collins.

CONTINUING a communication made in 1869 to the Philadelphia Academy of Sciences, Dr. Lea, in this pamphlet, gives the results of further examination of the crystals and cavities to be found in gems and minerals. His special researches are illustrated by a plate in which are represented cavities of all shapes, with and without fluid contents, crystals of various shapes and maculations in corundum, sapphire of different shades, zircon, moonstone, emerald, where the cavities contained cubic crystals surrounded by a fluid, and beryl with irregular imperfections. The microscopic study of gems must possess great interest to any one whose opportunities allow it.

ESSAYS ON MIND, MATTER, FORCES, THEOLOGY, ETC. By CHARLES E. TOWNSEND. New York: Charles P. Somerby. Pp. 404. Price, $2.

THE papers which make up this book originally appeared in the *Phrenological Journal* and other publications, and embrace discussions on subjects relating to physics, astronomy, biology, social science, religion, etc. "The essays are chiefly intended to uphold the theory of the stability of matter and forces, and the perpetuity of all minds, as material forces, on a new basis of reasoning, in opposition to the many present vague theories of spirit-minds. Also, as opposed to the assumed origin of matter from nothing, and its inevitable extinguishment in time—not mere change of form and action, but utter annihilation being claimed by some." The author vehemently opposes the "debasing, stagnant theology of over eighteen centuries," deprecates the "folly of Biblical cant," and believes that the "Christian religion is an old-times crude theology and false cosmogony, that ought to be replaced by a more rational and ennobling conception and worship of an infinitely intelligent great First Cause, who is known to us through his creations, and thus inferred attributes of infinite goodness, wisdom, and power."

Various theories are presented in regard to different subjects, which are not wonderful so much for their novelty as for the obscure manner in which they are stated.

There is an air of ultimate truth assumed throughout the essays, which the conclusions hardly warrant; and the author would probably write a better book if he exercised his dogmatic tendencies less and cultivated a clearer style more.

REPORT OF THE CONDITION OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, ON MOVING INTO ITS NEW EDIFICE, SOUTHWEST CORNER OF RACE AND NINETEENTH STREETS. By W. S. W. RUSCHENBERGER. Philadelphia: Collins, Printer. Pp. 56.

THE Academy of Natural Sciences of Philadelphia dates from the year 1812, and, at the close of that year, consisted of fourteen members, who assembled on the second floor of a house devoted to millinery purposes. Although their progress was slow, yet, in 1817, the publication of their *Journal* was commenced, and in 1820 they sought more spacious accommodations in a Swedenborgian church. Twenty years later a new building was erected, more space was given to the collections, and an increased number of visitors continued to be attracted. It again outgrew its quarters, and ten years ago a movement was started which resulted in the present edifice. The Academy is now free from debt; it possesses a building constructed with reference to architectural beauty and to the ends for which it was designed, and is apparently in a very flourishing condition. Its cabinets of birds and shells of mollusks are nowhere surpassed in extent and completeness, and in other departments the collections are valuable, though, as yet, comparatively small.

THE STRUCTURE AND RELATIONS OF DINICHTHYS; with Descriptions of Some Other Fossil Fishes. By J. S. NEWBERRY. Columbus: Nevins & Meyers.

DR. NEWBERRY has reprinted this memoir from Vol. II. of the "Report of the Geological Survey of Ohio," of which we gave a notice last month, and it is accompanied by seven lithographic plates and many woodcut figures. The *Dinichthys*, to which the body of the pamphlet is devoted, is a huge ganoid fish, occurring along the Lake Erie shore in the Huronian shales, and peculiar among its allies in its massive mandibles and in its dentition, which closely resembles that of living *Lepidosiren*. Other resemblances between them are so close as to warrant the belief that in the *Lepidosiren* we have a dwarfed representative of the great fishes which populated the Devonian seas. Dr. Newberry discusses minutely the anatomy and relationships, homologically and generally, of these monarchs among ancient fishes, and describes several additional species. The latter half of the book is occupied with descriptions of new fossil fishes from the carboniferous rocks of Ohio, belonging to various orders and families, all the points of which are elaborated with the close attention characteristic of this distinguished geologist.

IN a paper read before the Detroit meeting of the American Association, and now reprinted, Prof. Aug. R. Grote explained the effect of the glacial epoch on the distribution of insects in North America. He endeavors—successfully, we think—to show that arctic forms of insects, the White Mountain butterfly, for example, came southward with the gradual extension of the ice-sheet, and, when the ice-sheet retreated, followed it backward; but some, straying away, or lingering about the local glaciers of high mountain-ranges, gradually followed the declining cold to the high summits, where only could they find a congenial climate. Meanwhile, the surrounding lowlands having become warm, they could not follow their congeners to the arctic zone, but were imprisoned, as it were, on their mountain-tops, and have there remained, undergoing modifications caused by the exigencies of their surroundings. Some such process, Prof. Grote judges, has determined the distribution of most of our Alpine insects.

THE Report of the Director of the Central Park Menagerie, Mr. W. A. Conklin, for the past two years, shows that, in spite of the lack of encouragement afforded it by the Park Commissioners, that commendable institution continues prosperous, and is visited by increasing crowds of spectators—among others whole schools, with their teachers, attesting its educational value. The appropriations for it allow of little more than the care of the inmates, but many animals are received on deposit from their owners, and births are constantly oc-

curring. The mortality is low, and becoming less so, owing to improved arrangements and more commodious quarters. The Report contains the usual details of expense and management, and a long list of accessions, which might be of use to visitors in lieu of a guide to the menagerie.

THE TEXTILE COLOURIST: A Monthly Journal of Bleaching, Printing, Dyeing, and Finishing Textile Fabrics, and the Manufacture and Application of Colouring Matters. Edited by CHARLES O'NEILL, F. C. S. Price, $12 per annum.

"The Textile Colourist" was designed by its present editor to bring before the dyers and printers of the different countries such matter as will be of a permanently interesting character to all in the trade. There are embodied in it the results of the most recent investigations and discoveries, arranged in such a manner as to make it a valuable work of reference.

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PUBLICATIONS RECEIVED.

Tolhausen's Technological Dictionary, French, German, and English. 3 vols., 900 pages each. New York: H. Holt & Co. Price, $3.50 per vol.

The Electric Bath. By George M. Schweig, M. D. Pp. 134. New York: Putnam's Sons. Price, $1.

Improvements of the Fox and Wisconsin Rivers. By G. K. Warren, Brevet Major-General. Pp. 114, with Plates.

Qualitative Chemical Analysis. By Douglas and Prescott. Pp. 254. New York: Van Nostrand. Price, $3.50.

Geological Survey of the Territories. Vol. X. F. V. Hayden, Geologist in Charge. Pp. 607, with plates. Washington: Government Printing-Office.

Department of Agriculture. Report of 1875. Pp. 536, with plates. Washington: Government Printing-Office.

Calendar of the Tokio Imperial University (1876). Pp. 165.

Preventing the Extension of Syphilis. By J. R. Black, M. D. Newark, Ohio. Pp. 7.

Memorial of Increase A. Lapham. By C. Mann. Pp. 21.

Topographical Surveys and the Public Health. By J. T. Gardner. Pp. 10. Albany: *Argus* print.

Needs of the South educationally. By A. Hogg, M. A. Pp. 24. Salem, Ohio: W. D. Henkle print.

Quarterly Journal of Inebriety. Vol. I., No. 1. Pp. 64. Hartford: Case, Lockwood & Brainard.

History of Spontaneous Generation. By E. S. Dunster, M. D. Pp. 30. Ann Arbor, Michigan: *Courier* print.

Reason and Progress. By J. T. Stewart, M. D. Pp. 18. Peoria, Illinois: *Transcript* print.

Treatment of Eczema. By R. W. Taylor, M. D. Pp. 37. New York: Putnam's Sons.

Hydroadipsia. Pp. 9. Also, The Fever Process in Human Bodies. Pp. 7. By Z. C. McElroy, M. D. Zanesville, Ohio.

Disinfection in Yellow Fever. By C. B. White, M. D. Pp. 16. New Orleans: J. W. Madden print.

Rocky Mountain Locust. Pp. 58. St. Louis: R. P. Studley Company print.

Specialism in Medicine. By E. D. Forée, M. D. Pp. 10. Indianapolis: *Journal* print.

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## POPULAR MISCELLANY.

**Talking by Telegraph.**—On Sunday, November 26th, Prof. A. Graham Bell experimented with the "telephone" on the wires of the Eastern Railroad Company between Boston and Salem. Prof. Bell was assisted at the Boston end of the line by two operators, and Mr. Thomas A. Watson by one operator at the Salem end. According to the account published in the *Commonwealth* of Boston, conversation was carried on with Mr. Watson at Salem, by all those present, in turn, without any difficulty, even the voices of the speakers being easily recognized. Whispering was found to be perfectly audible, but was unintelligible. After a time, instead of grounding the wire at Salem, it was connected with North Conway, a distance of one hundred and forty-three miles from Boston, thus leaving Salem as a way-station. After this change had been

made there was a slight diminution in the loudness of the tones, but no difficulty was experienced in carrying on conversation. Another change was made, whereby the electrical current was sent to Portland and back by another line to Salem, thus making Salem a terminal station at the end of nearly two hundred miles of wire. The result of this change was, that the tones of the speakers could be heard, but so faintly as to be unintelligible. With electro-magnets of a higher resistance, Prof. Bell is confident that the sounds would have been perfectly intelligible, the magnets used, it must be recollected, being only intended for a twenty-mile circuit.

**How to reach the Pole.**—Captain H. W. Howgate, of the Signal-Office, sees no grounds of discouragement in the failure of Nares's expedition to reach the north-pole. The seasons, he remarks, vary in the arctic circle as markedly as in more temperate latitudes, and in a favorable year the ice of the so-called "Palæocrystic Sea" might be broken up. Captain Howgate would have a party of at least twenty hardy, resolute, and experienced men, with provisions for three years, stationed at some point near the borders of the Polar Sea—for instance, where the Discovery wintered last year. These men would seize the occasion of the opening of the frozen sea to push on to the pole. At the end of three years the party should be visited, and, if unsuccessful in accomplishing the object, should be revictualed and again left to their work. With a good, substantial building, such as could easily be carried on shipboard, they would be as comfortable and safe from atmospheric danger as the men of the Signal Service on the summit of Mount Washington. "A good supply of medicine," adds Captain Howgate, "a skillful surgeon, and such fresh provision as could be found by hunting-parties, would enable them to keep off scurvy, and to maintain as good a sanitary condition as the inhabitants of Godhaven in Greenland. Game was found in fair quantities by the Polaris party on the Greenland coast, and by those from the Alert and Discovery on the mainland to the west, especially in the vicinity of the last-named vessel, where fifty-four musk-oxen were killed during the season, with quantities of other and smaller game. A seam of good coal was also found by the Discovery's party, which would render the question of fuel a light one, and thus remove one of the greatest difficulties hitherto found by arctic voyagers. Let an expedition be organized to start in the spring of 1877, and I firmly believe that by 1880 the geography of the polar circle would be definitely settled, and that without loss of life."

**Classification of the Races of Man.**—The distinguished Italian ethnologist, Prof. Mantegazza, of Florence, in his introduction to Enrico Giglioli's narrative of a voyage round the Globe in the corvette Magenta, learnedly discusses the question of the classification of the races of man. His principal conclusions are that—1. Man is one of the most cosmopolitan and most variable of animals, and hence presents an infinite variety of races, sub-races, and peoples. 2. The number of races is indefinite; many races are extinct, others are now forming, still others will yet be produced. 3. The farther back we go in history, the larger is the number of races and sub-races, for in early times men less frequently moved away from their native localities and were more isolated from one another than now. 4. At the top and at the bottom of the human genealogical tree the branches and twigs approach one another, so that the most highly-cultured and the least developed races come into mutual contact. The negro developed into a Kaffre approximates to the European, and the European, degraded by cretinism or by hunger, to the Australian or the negro. 5. In general the lowest races are black or dark brown, the middle races somewhat less dark-skinned, and the highest white or nearly so. 6. In classifying the races of man we must, as far as possible, omit the question of their origin, for the investigation of this origin is the most fruitful source of ethnological errors.

**The American Geographical Society.**—The American Geographical Society was formally installed in its new quarters, No. 11 West Twenty-ninth Street, New York, November 28th. For many years this Society

had rooms in the Cooper Union Building, but from the beginning it has been the intention of the leading members to secure possession of a building large enough to receive their valuable collection and library. The new headquarters is a large four-story and basement brown-stone front house on Twenty-ninth Street, near Fifth Avenue. On the first floor is a spacious reception-room, extending the entire depth of the house; its walls are covered with maps and charts. One of the curiosities of this room is the large map of South America once used by Humboldt. On this floor is also the room of the president of the Society. The second floor is devoted to the library and the secretary's room. In the library are 20,000 volumes, classified according to countries. The third floor contains the collection of maps and atlases. On the fourth floor is the Council's room, and in the basement are the offices for the clerical force.

On the evening of November 29th a second reception was held by the Society, and a paper on a journey to the Spitzbergen Sea was read by A. H. v. d. Hoeck. The author took occasion to expatiate upon the value of arctic research, pointing out the important results thence to be derived for anthropology, zoölogy, geology and paleontology, physics, and meteorology. Manuel M. Pereira, minister resident of Costa Rica, read a short paper on the projected canal across the Isthmus of Darien.

**Hygrometers.**—An hygrometer is an instrument for measuring the moisture of the atmosphere. It is often useful for gauging the dryness of rooms. It may not be generally known how simply such an instrument may be constructed.

When water, by means of a moist rag (whose moisture may be kept up by contact with water in a saucer or teacup), is spread over the surface of the bulb of a thermometer, the mercury in the latter falls, generally several degrees. The reason is, that the water evaporates and cools the bulb. The evaporation which takes place is, of course, produced by the absorption of heat from surrounding objects, the bulb included. The thermometer is affected in proportion to the reduction of temperature caused by the evaporation. It is evident that just as much heat as is required to convert water into vapor, just so much cold (or deprivation of heat) will be required to convert the vapor back again into water. When vapor begins to condense into water the temperature is at what is called the dew-point. It is evident, therefore, that theoretically the dew-point is twice as far as the vapor-point below the normal temperature of the atmosphere. Experiments show that it is a little more; a constant quantity of $1\frac{1}{3}°$ Fahr. having to be added for heat lost and dissipated in the process.

These facts may be exemplified as follows: Hang two thermometers in a room of equable temperature, and suspend a third in a tin or glass vessel containing some tepid water. Wet the bulb of thermometer No. 2 as suggested above, and the evaporation will show the vapor-point. Pour ice-water gradually and slowly into the vessel containing No. 3, and mingle it well with the water already there until the whole becomes so cold that the exterior of the vessel begins to contract moisture. It is then at the dew-point, and the thermometer in the vessel will be found to have fallen twice as much as No. 2, and $1\frac{2}{3}°$ more.

No. 2 is a perfect hygrometer, as it shows the relative dampness of the atmosphere. When the latter is very dry, as in a room warmed by a hot-air furnace, evaporation takes place rapidly, and a large quantity of heat is abstracted from the bulb. When moist, as during a shower, very little evaporation takes place, and there is but a slight fall of the hygrometer. When the atmosphere is too dry the lungs suffer. It is in a wholesome condition when the hygrometer does not fall more than 7° Fahr. below the normal temperature. A hot-air furnace often sends it down 10° or 12° below.

**The Studies of an Engineer.**—Prof. Reynolds, of Owens College, Manchester, in an address on "Engineering as a Profession," proposes the following course of preparation for the student who aims to be an engineer: Up to the age of sixteen or seventeen he should devote himself to acquiring a "general education." Then he enters on his special course. In this he must learn something of science and something of art;

but his main object should be to learn how the one can be brought to bear upon the other. Mathematics and natural science are indispensable, but he must not expect to become a master of either. Only a comparatively small portion of these wide subjects can be usefully brought to bear on engineering, and to these he must restrict himself. The methods of applying these sciences to engineering problems constitute a large subject, and one that is necessary for him to study. Then there are those manual operations which are essential to bring his knowledge to a practical issue, and in which a long course of training is necessary to acquire the requisite skill, such as mechanical drawing, and the use of measuring and surveying instruments. To acquire a useful knowledge of these various branches will require three or at least two years. The student will then proceed with his practical training, which should include as great a range of work as possible. In this he will find the knowledge he has acquired of very great help; he will recognize much that he sees, and be able to judge of the most important things to which to direct his attention.

**Absorption of Nitrogen by Plants.**—The chemist Berthelot has submitted to the Paris Academy of Sciences the results of a new series of experiments which prove that, under the influence of atmospheric electricity, free nitrogen is absorbed by the proximate principles of plants. The apparatus used in these experiments consists of a system of tubes in which the organic substances come into contact either with pure nitrogen or with atmospheric air, the whole communicating with a source of electricity at a tension precisely the same as that of atmospheric electricity. Under these conditions pure nitrogen, or the nitrogen of the atmosphere, is invariably fixed by the organic matter employed, viz., wet filtering-paper or a solution of dextrine. The amount of nitrogen that is thus fixed is considerable. Thus these experiments bring to light a natural cause, hitherto overlooked, in considering the question of the fixation of nitrogen by vegetable tissues. It is now demonstrated that this fixation is brought about by the incessant action of the electricity of the atmosphere.

**Development of Electricity by Light.**—To determine experimentally the action of light in the development of electricity, Hankel took two bright strips of copper, one of which he fixed in a porous clay cell by means of a cork stopper. The cell was filled with water, and placed in a larger glass vessel containing the same water, in which was immersed the other strip, so that one of its surfaces was turned toward the source of light. The two strips were connected with the wire of a galvanometer. The glass with its contents was now placed in a black case having a slide, by means of which direct sunlight or colored light could be admitted to the outer strip of copper. The results were as follows: On access of free sunlight the illuminated strip was negative to the one in darkness, but only moderately so; behind a red glass the action was extremely small; behind yellow, a little stronger; behind green and dark blue successively, still stronger; behind dark violet it became less again.

The copper strips were now oxidized by moderate heating, and the following results were obtained: In free sunlight the illuminated strip was strongly negative; on darkening again, the deflection gradually disappeared; behind red glass the action was less; behind light-yellow glass the plate was first positive, then negative; on darkening, it first became still more negative, and then the action disappeared; behind dark-green glass the behavior was similar, but the first positive deflection was less; behind bright-blue, dark-blue, and violet glass, the plate was equally negative.

Strongly oxidized copper strips were next tested. In free sunlight the illuminated strip was first strongly positive, then weakly negative; on darkening, it was first strongly negative, then the action ceased. Behind red glass the plate was pretty strongly positive, but the deflection of the needle soon fell off considerably; behind bright-yellow glass the strip was very strongly positive, but very soon the action diminished; on darkening, a strong negative deflection occurred. Behind dark-green glass the plate was first weakly positive, and then negative; behind dark-blue glass the cop per was also negative, and this change was more considerable than with free sunlight; behind violet glass, the action was similar.

The author describes also the behavior of copper in sulphate-of-copper solution, and the behavior of silver, tin, brass, zinc, platinum; which metals were examined in the same way.

**American Vine-Stocks and the Phylloxera.**—In a communication to the Paris Academy of Sciences, M. Boutin gives an account of researches made by him to ascertain the reason why some American vine-stocks resist the attacks of the *Phyllozera vastatrix*, while others succumb. The author has discovered in the resistant stocks a certain resinoid principle in proportion about a third greater than that in which it occurs in American non-resistant stocks, and in about double the proportion found in French stocks. M. Boutin considers it essential for resistance that the resinoid principle should occur in the proportion of 8 per cent. for the entire root, and 14 to 15 per cent. in the bark alone. He says that the incision made by the insect, while producing nodosities in the root, is cicatrized by the exudation of the resinous product, and this prevents the escape or loss of the nutritive sap of the plant. In non-resistant stocks, on the other hand, there is no cicatrization, as the resinoid principle is not in sufficient quantity to produce this effect.

**Naturalists' Report of the British Arctic Expedition.**—The results obtained by the naturalists attached to the British North-Polar Expedition may be briefly summed up as follows: The mammals found farthest north, on the shore of the great Polar Basin, were the arctic fox, wolf, ermine, polar hare, lemming, and musk-ox. Bird-life was present as far as the land extended, the outlying species being the snowy owl, snow-bunting, and ptarmigan. Of fishes few marine species were procured, but an interesting small salmonoid was found in fresh-water lakes up to about latitude 82° 35′. Insect-life was more abundant than could have been expected, and a goodly number of species were obtained. Over twenty species of phænogamic plants were discovered between latitudes 82° and 83°, and the cryptogamic flora was of course much more varied and abundant. The whole west coast of Smith's Sound, from Cape Isabella to Cape Union, was fully surveyed and mapped, and large collections made of both fossils and rock-specimens; while the sled-parties, which explored the shores of the Polar Basin both to east and west, brought back sufficient material to determine the geological character of the country. Silurian limestones, richly fossiliferous, were the prevailing rocks along Smith's Sound. From the shales and sandstones of this formation a beautiful series of leaf-impressions were collected, illustrating the characteristic flora of the epoch, and presenting a remarkable demonstration of the existence of a temperate climate within 500 miles of the present pole, at a comparatively recent geological time. Lastly, very interesting and suggestive observations were made on glaciation and on ice-action in general.

**The Berlin Gorilla.**—At the recent meeting of the German Association of Naturalists, Dr. Hermes, as we are informed by *Nature*, described some interesting characteristics of the young gorilla in the Berlin Aquarium. He nods and claps his hands to visitors; wakes up like a man and stretches himself. His keeper must always be beside him and eat with him; he eats what his keeper eats; they share dinner and supper; the keeper must remain by him till he goes to sleep, his sleep lasting eight hours. His easy life has increased his weight in a few months from thirty-one to thirty-seven pounds. For some weeks he had inflammation of the lungs, when his old friend Dr. Falkenstein was fetched, who treated him with quinine and Ems-water, which made him better. When Dr. Hermes left the gorilla on the previous Sunday, the latter showed the doctor his tongue, clapped his hands, and squeezed the hand of the doctor as an indication, the latter believed, of his recovery. For Pungu, as the gorilla is called, a large plate-glass palace has been erected in the aquarium in connection with the palm-house.

**Lightning in a Telegraph-Office.**—A telegraph-operator, in an office on the Boston and Providence Railroad, was lately killed by lightning. This is said to be the only case on record of an operator killed by lightning while in the office. Remarking upon this casualty, the *Telegraphic Journal* says that,

"so far from being a source of danger, the electric telegraph must be regarded rather as a cause of safety, as a network of lines spread over a country tends to prevent an accumulation of electricity at any particular point, by continually and silently discharging it to the earth. This is particularly the case in districts where every pole has an earth-wire fixed to it, running from the top to the bottom. That these wires effectually discharge a lightning-flash has been seen in cases where the wires have been terminated within a few inches of the top of the pole: a lightning-flash striking one of these destroyed the portion of pole above the wire, but at the point where the wire commenced all damage ceased."

## NOTES.

UNDER the head of "Commercial Manias," we referred last month to the banking enterprise of a lady at Madrid. The *Economist* of December, 1876, reports further on the case, as follows: "The extraordinary banking at Madrid, by a lady who paid twenty per cent. interest monthly on deposits, has ended in a manner which has long been expected. She disappeared a few days back with a sum of three million and a half of francs ($700,000), out of five million and a half ($900,000) she had received from 6,700 depositors. The difference had been returned in interests."

RECENT additions to the museum of the Buffalo Society of Natural Sciences embrace a small collection of European *Lepidoptera*, presented by Prof. Zeller, of Stettin; a few rare specimens from the Museum of Comparative Zoölogy, and a collection of California *Geometræ* from Prof. Behrens, of San Francisco; also a collection of butterflies and moths from Mr. John D. Shepard, taken on the Wahsatch Mountains, twenty-five miles south of Salt Lake City.

AN association of ladies has been formed in Boston, entitled the Boston University Women's Educational Society, for the purpose of promoting the higher education of women. The Society proposes to aid needy students by gifts and loans, and also to found resident and traveling fellowships, to encourage original research, and in general to afford to young women all the educational facilities now accessible only to young men. A fund amounting to $40,000 has already been accumulated.

A SMALL coleopterous insect (*Anthrenus scrophulariæ*) common in Europe, but hitherto unknown in the United States, has made its appearance in the neighborhood of Albany. The larva of this insect is a great destroyer of clothes, furs, natural-history collections, etc., and at Albany much damage has been done by it to carpets.

THE Litchfield Astronomical Observatory, of Hamilton College, of which Prof. C. H. F. Peters has been for nearly twenty years the very efficient director, has lately been enlarged. Efforts are being made to retain the services of Prof. Porter as assistant astronomer.

DIED, October 19th, at the age of fifty-four years, Carl Jelinek, for thirteen years director of the Vienna Central Institute for Meteorology and Magnetism. His papers on Austrian meteorology are held in very high esteem, and his "Introduction to Meteorological Observations" has reached the third edition.

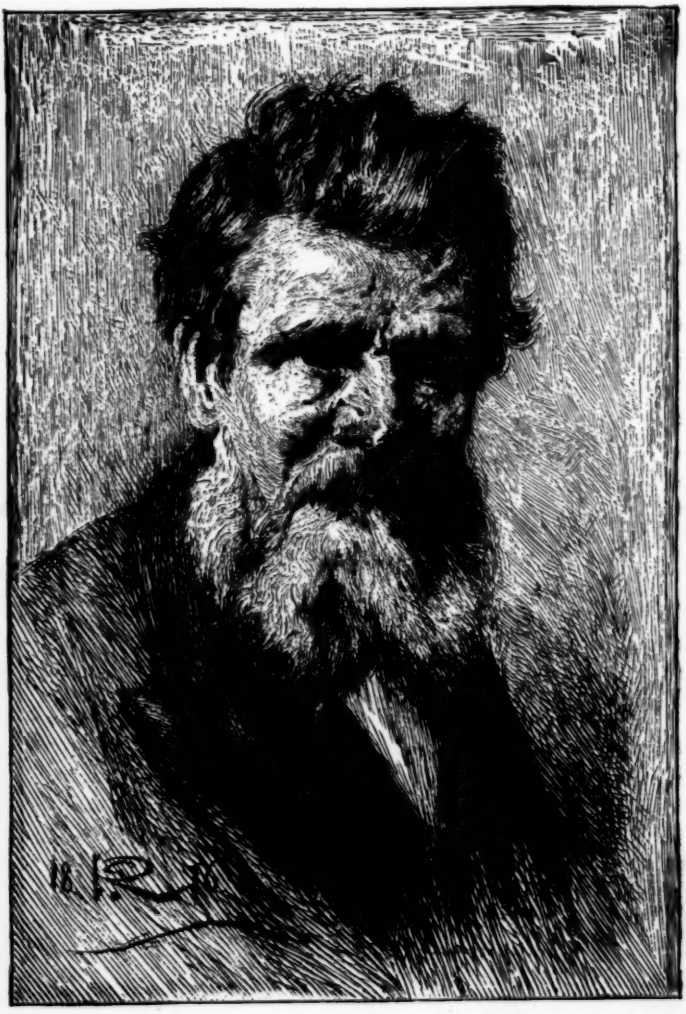
THE *American Chemist* for September announces the discovery of a new element by Dr. George A. Koenig, of the University of Pennsylvania. From a mineral resembling schorlamite, occurring at Magnet Cove, Arkansas, he obtained, in the place of titanic acid, a *white oxide*, which differs from the former very materially, and he regards the existence in it of a new metal as highly probable.

THE subscription for a monument to Liebig in Germany has reached the sum of 140,000 marks, and the lists are now closed. Giessen and Munich claim the statue each for itself. It has been decided that both towns shall have the same memorial, which will be cast in bronze, the sum collected being sufficient to cover the double expense.

WILLIAMS COLLEGE will next summer send an exploring party to the Rocky Mountains, under the lead of Prof. Sanborn Tenney, Professor of Natural History. The party will consist of fifteen students, who, during the remainder of the college year, will receive special instruction to fit them for the performance of their respective duties.

THE statue of Faraday, the commission for which was placed in the hands of the late sculptor Foley, and which was far advanced by him in the full-sized model at the time of his death, has been completed in marble by one of the disciples of the deceased artist, and is now awaiting arrangements for erection.

IN an article on the scurvy which broke out among the sled-parties of the British Polar Expedition, the *Sanitary Record* says that never were the plainest results of past experience and the best-established rules of naval hygiene more recklessly and disastrously set at naught than in these sled-expeditions.



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